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Edited by
JOHN B. WATSON
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Volume 2, Number 1, 1913 Serial Number 6

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The Johns Hopkins University

The Delayed Reaction in Animals and Children

WALTER S. HUNTER

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Volume 2, Number 1 Serial Number 6

Edited by JOHN B. WATSON
The Johns Hopkins University

Studies from the Psychological Laboratory of the University of Chicago

The Delayed Reaction in Animals and Children

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Instructor in Philosophy, The University of Texas

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at Cambridge, Boston, Mass.
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TABLE OF CONTENTS

Page

I. Introduction 1

II. Critical Review of Historical Data 2
1. Imitation 3
2. Use of tools 5
3. Dreams 8
4. Learning curve 8
5. Memory 9
6. Thorndike's test 10
7. Learning by being "put through" 14
8. Recognition 16
9. Rate of forgetting 16
10. Association by similarity 16
11. Reluctance and expectancy 17
12. Varying means to the same end 17
13. Reactions to a temporal series of colors 19
14. Washburn's cat on the stairway 20

III. Notes on the Animals and Children Tested 21

IV. Apparatus and General Method 22

V. Experimental Results 30
1. Tests with animals 30
A. Learning the association 30
(a) Rats 30
(b) Dogs 32
(c) Raccoons 33
(d) Summary 34
B. Maximal intervals of delay attained 35
(a) Group differences in maximal delay 35
(b) Effect of size of release upon delay 37
(c) Effect of backgrounds of different brightnesses upon delay 38
(d) Effect of number of boxes upon delay 39
(e) Effect of other conditions upon delay 39
C. Methods of reaction after delay used by animals 39
(a) Orientation of whole or part of body 40
(b) Position in the box 47
2. Tests with children 52
A. Method of experimentation 52
B. Are the results obtained from animals and children comparable? 53
C. Learning 55
D. Differences between the learning of animals and children ... 55
E. Delayed reactions 57

VI. Theoretical Considerations 62
1. The cues essential for correct delayed reactions 62
A. Substitutes derived from the external environment 63
B. Substitutes derived from within the subject's body 67
(a) The type of internal cue used 67
(b) The mechanism by which internal cues guide behavior 70
2. The place of ideas in the grades of animal learning 73

VII. Summary and Conclusions 79

VIII. Appendix 82
A. Detailed records of two rats and two raccoons 82
B. Notes on raccoons 85

I. INTRODUCTION

The experiments in this monograph [1] aim at an analysis of typical mammalian behavior under conditions where the determining stimulus is absent at the moment of response. Associations were first set up between movements that led to food and a light which might be in any one of three boxes. Controls were used to make sure that the position of the light alone determined the reactions of the subject. Tests were then instituted in which the light was turned off before the reaction was made. The subject thus had to respond in the absence of the stimulus that hitherto had guided his reactions.

The nature of the present experiment may be further set forth by contrasting it with the following type of adjustment: A cat watches for a mouse and sees it appear at an open hole. The mouse vanishes before the cat can react, yet the cat goes over to the hole. There can be no question here but that the determining stimulus is absent at the moment of response, provided possible olfactory stimuli be neglected. Our experiment differs from this in complexity. If there were three holes that differed only in their several directions from the cat, and if in the past the mouse had appeared an equal number of times in all three holes, the conditions would be the same as in our tests. A selection between the three holes would need to be made on the basis of the immediately previous presence of the rat, if a correct reaction were to occur. If an animal *can* manifest behavior that does not lend itself to a "stimulus and response" explanation, this is one type of situation in which that behavior should appear. That, in fact, it is the situation *par excellence* for the eliciting of this behavior will, I believe, appear as this monograph progresses.

[1] Experimentation on the present problem was first begun in the University of Chicago laboratory by a graduate student, W. R. Hough. The following year the work was taken up and carried somewhat further by another student, H. B. Reed. Both students worked with white rats. Although in each case the results obtained were in strict harmony with those presented in this paper, in neither case were they conclusive. The chief value of the work lay in its suggestiveness. The apparatus used by Reed—Prob. Box D—is described below. The present investigation was carried on in the same laboratory from October, 1910 to April, 1912.

In the present experiments, two main factual questions arise: (1) How long after the determining stimulus has disappeared can an animal wait and still react correctly? (2) Does the animal give any behavior cues as to its method of solving the problem? If so, what are they? With these data given, there remains the task of interpretation. If a selective response has been initiated and controlled by a certain stimulus, and if the response can still be made successfully in the absence of that stimulus, then the subject must be using something that functions for the stimulus in initiating and guiding the correct response. Our investigation thus forces us to the consideration of the functional presence of a representative factor in the behavior of animals and children. Not only this, but the problem of the nature of this representative factor confronts us. Is it an overt motor attitude, or not? If not, is it sensory or imagnal, i.e., ideational?

In the interpretative study, I shall proceed on the assumption that animals are conscious. What the nature of this consciousness is, it will be the task of this paper to help determine. (If the reader does not choose to follow this line of interpretation, he may state everything in neurological terms without marring the significance of this discussion.) But *a propos* of the term "image" or "idea," let it be said once for all that wherever these terms are used by the present writer with reference to animal consciousness, they should be supplemented by the phrase "or functionally equivalent process." I use the structural term chiefly for the sake of its brevity.

II. CRITICAL REVIEW OF HISTORICAL DATA

In the interpretative discussion at the close of the present monograph, we shall be confronted with the possibility that images or ideas [2] may have guided the reactions of the subjects. In that discussion, we shall assume that there is no necessity that psychology postulate such a representative factor save where successful reactions occur in the *absence* of the stim-

[2] In the literature, it has been taken for granted that the question of the existence of images and that of the existence of ideas in animal consciousness is the same. I shall proceed on the assumption that images are centrally aroused processes, while ideas may be either peripherally or centrally conditioned. The essence of the idea is not its origin (or content), but its function. This point will be amplified in the final divisions of this monograph. The present statement will suffice for a definition of terms.

ulus (object) or movement that is represented. In the present historical section the attempt is made: (1)To present the main types of behavior that have been used as evidence of the existence of images in animals; and (2) to show that this evidence is inconclusive, because the behavior may be stated in sensori-motor terms. In the light of the assumption just made, there are two reasons why this behavior may be so interpreted: (1) The stimuli determining the reactions in question have not been adequately known; and (2) inasmuch as this is true, one has no right to assume the absence of the stimuli which are supposed to be represented. All of the behavior that is summarized in this section *may* have involved representative actors. The point of my criticism will be that they *need* not have involved such processes. It is not merely a question of the application of the law of parsimony, as that is usually stated. It is also a recognition of the fact that sensori-motor behavior is genetically the more fundamental form.

The following are the types of evidence to be considered: (1) Imitation; (2) use of tools; (3) dreams; (4) nature of the learning curve; (5) memory; (6) Thorndike's test: The immediate reaction to one stimulus before the appearance of a second which has always accompanied the first after an interval of a few seconds; (7) recognition; (8) learning by being "put through;" (9) rate of forgetting; (10) association by similarity; (11) reluctancy and expectancy of response; (12) varying means to the same end; (13) reactions to a temporal series of colors, and (14) Washburn's cat on the stairway.

1. Imitation

Thorndike [3] is an example of the comparative psychologist who interprets the highest type of imitation as requiring the presence of images or ideas. In his experiments, however, he found no evidence for the existence of this kind of behavior. The general character of the imitation experiment is well known. An animal that has failed to solve a certain problem is confined where it can see another work the mechanism and get food. It is then given an opportunity to make the desired reaction. If the trial succeeds or if there is a sudden improvement in the animal's behavior, it is said to have profited by

[3] Thorndike, E. L. Animal Intelligence. 1911, New York, pp. 76–108.

viewing the other's performance. Haggerty [4] found this behavior in well controlled tests with monkeys. He divides the stimulus for that imitative behavior into three parts: (1) A fairly simple mechanism; (2) the perception of another animal working at the mechanism, and (3) the perception of the other animal getting food. Haggerty does not discuss the theoretical side of imitation and argue for the existence of images. He simply presents the data. However, in view of the stand taken by Thorndike and others on interpretation, we shall nevertheless use material drawn from Haggerty's monograph for analysis.

The following criticisms tell against the use of this type of behavior as crucial evidence for images: The animal that does the imitating may make its improvement under the incentive of a social impulse rather than of the apprehension of relations. This receives confirmation by the fact that Haggerty's monkeys did better if the animal to be imitated was a stranger and thus aroused the imitator more strongly than a familiar animal would have done.[5] This criticism is a variant on the following one. (2) The fact that the sight of the other animal *being fed* was a part of the stimulus makes it at least possible that the imitator's attention was simply drawn very vividly to the spot to be attacked. Of course the more complicated the reaction to be made and the more exact the execution of this by the imitator, the better the argument for an ideational perception of relations. However, the general objection will always be valid that one can never say that the imitating animal was not guided solely by stimuli present to sense. The determining factor in the stimulus can not be said to be ideational. Once the animal's attention is vividly focused upon the objective sequence "pulling string—getting food," e.g., the necessary reaction may follow by association when the opportunity presents itself. Monkeys in particular have such an enormous repertoire of reactions that it is very tempting to analyze such imitative reactions as Haggerty presents on a purely "stimulus and response" basis. That author says of one of his typical experiments (Chute Experiment A): "In order to secure the food, the monkey must leap from the wire part of the cage to the chute, and, while

[4] Haggerty, M. E. Imitation in Monkeys. *Jour. of Comp. Neur. and Psy.*, 1909 vol. 19.

[5] Op. cit., p. 436.

holding to it, must thrust a hand up inside and pull the string, thereby releasing the small door in the top of the cage and allowing food which had been placed on it to fall to the floor. He must then descend to the floor to get the food." [6] *After the imitator's attention is focused upon the hole at the chute's end* due to the action of the imitatee, what is more natural than that it should jump to the chute? The hole has "caught its eye" before the jump, so the hand goes in almost reflexly, as reflexly closes about the string and pulls. Food falls and is eaten. If the animal is attentive to the movements during the performance, the chances are that it will henceforth succeed.[7]

2. Use of Tools

A second argument for the presence of ideas is the use of tools by animals. In view of the fact that such behavior has at times been exclusively claimed for man, students of animal behavior have long sought for conclusive evidence on this question.[8] We shall take typical data presented by Hobhouse for consideration. This will also lead to an understanding of that author's attitude on the question of images. Hobhouse treats such behavior largely under the heading "Articulate Ideas." An example will best lead us to an understanding of his position. The animals used were monkeys, one a Rhesus and the other a chimpanzee. The latter had already learned to throw a rug over food placed at a distance in order to rake in the latter. He was then taught to substitute a stick for the rug. Quotations from the author will now indicate the animal's progress. "Next day, the chimpanzee learnt to use a short stick in order to reach a larger one, with which in turn he could reach the banana." [9] "One day I gave him a rope with a noose to throw over the box in place of his stick." (The banana was placed in a cigar box.) "I did not give him any hint, but he soon tried it in a vague way. He did not, however, understand the matter very well, for when he succeeded in getting the

[6] Op. cit., p. 355.

[7] Vide Hobhouse, L. T. Mind in Evolution. London, 1901, p. 202 for place of accident in learning.

[8] Lindsay, W. Lauder. Mind in the Lower Animals in Health and Disease. London, 1879, vol. I, chaps. 23 and 24.
Hobhouse, L. T. Op. cit., chap. 10.
Also many recent studies in imitation.

[9] Op. cit., p. 236.

rope round the box, he did not seem aware of his advantage, flung it away, went off for his shawl, and used it very successfully. I then tied a block of wood to the rope to assist in throwing it. He attempted this spontaneously, at first without success. Presently, however, he happened to pitch the block right into the box, which today was open, pulled it in, and got the banana. Notwithstanding this signal success, he never took to this trick." [10] "In the experiment to which I have already referred, when the box was tied to a rope, the further end of which was passed over a stanchion several feet from the cage, he failed, as I shall mention later, to find the right method, but was fertile in devising wrong ones. He would shake the rope violently, so that the banana would fall out of the box. He would then swing the rope to and fro, swishing the banana about from side to side, until by degrees it would come within his reach, in a way which I should have thought beforehand to be quite impossible." [11] "In these tests," says Hobhouse, "it was necessary that [the monkeys] should grasp how the stick and the food stood in relation to them; that they should get the stick at the food and beyond it." [12] "A form of 'analogical extension' is also strongly marked in the use of substitutes differing very widely in appearance and the manner of use from the object first employed." [13] These illustrations of the use of tools are examples and proofs of the existence in monkeys of what Hobhouse means by the *practical judgment*, where articulate ideas are employed.[14] The author explains articulate ideas as follows: "By a more articulate idea, is meant one in which comparatively distinct elements are held in a comparatively distinct relation." [15] The nature of the practical judgment is set forth as follows: "It is more than assimilation, because what is revived is an idea, a definite reference to something unperceived. It is more than association, because relation between the 'revived' idea and the given perception is an essential part of it, and it is less than analytic thought, because the relations involved are not dissected out as distinct elements in

[10] Op. cit., pp. 237–8.
[11] Op. cit., p. 238.
[12] Op. cit., p. 241.
[13] Op. cit., pp. 241–2.
[14] Op. cit., p. 269.
[15] Op. cit., p. 234.

consciousness." [16] "The practical judgment is not independent of associations, for association supplies the whole of its material. But out of that material, it selects what it wants, and shapes it as required." [17] This "material" that association presents is ideational as the quotation from page 117 indicates. It seems, though I would not be sure, as if Hobhouse assumed the existence of ideas and is only concerned with the possibility of their functioning in behavior. As a basis for this imputation, I point to his discussion of the association of ideas on page 114 and to that on pages 200–1. In the latter place, he makes clear (?) that what he does not wish to attribute to animals is the conscious analysis of the perceptual order. Such a distinction between existence and efficient functioning in behavior seems to be theoretically permissible.[18] Images, ideas *may* exist sporadically as has been claimed in the case of animal dreams. But comparative psychologists have neither the right nor the need to assume the existence of such processes save as that may be forced upon them by the evidences of behavior.

The question now is: Does the *use* of tools[19] such as described imply the presence of a centrally aroused factor? In the light of the above analysis of imitation, the reply is no. Even if Hobhouse's results be fully accepted without raising the question of careful controls, one need not accept his interpretations. The behavior may have been controlled entirely by sensory factors. As to the fertility shown by the animals in devising methods, (see above, p. 6), this was very probably but a random use of acquired co-ordinations. The use of the tools was acquired as any habit and the sight of the individual objects (ropes, etc.) aroused the *type of reaction* that had been taught. This shows a higher grade of intelligent adaptability in the animal than if it had been limited to the use of one object, but it does not prove the existence of a central conscious factor. It may be that such animals as the primates are able to give similar responses to different sensory stimuli on account of a factor of "hyper-excitability." I hesitate to use such a term, but the general type of case held in mind is illustrated by the

[16] Op. cit., p. 117.
[17] Op. cit., p. 264.
[18] We shall see below (p. 9) that Morgan makes the same assumption.
[19] Hobhouse cities (op. cit., p. 258) only one well authenticated case of the making of a tool by an animal. The tool in this case was, as he notes, a very simple one.

animal that is supposed to react to a certain stimulus, but, being on the *qui vive*, reacts to anything that occurs at the proper moment. This behavior is familiar in human reaction experiments. If, now, we subtract the possible emotional disturbances (and I doubt whether that even is necessary), we have the type of case, I believe, that the experiments of Hobhouse present.

3. Dreams

A third argument used for ideas is the supposed fact of an mal dreams. The usual criticism of this—to which I subscribe—is that the law of parsimony forces one to recognise that the interpretation of the facts may as well or better be physiological than ideational.

4. Learning Curve

A fourth argument used in the discussion as to the presence of ideas is the nature of the learning curve. In 1898, Thorndike[20] presented data for cats which when plotted gave what the author termed a "gradual slope." "The gradual slope of the time curve,—shows the absence of reasoning." It represents "the wearing smooth of a path in the brain, not the decisions of a rational consciousness." There seems to be no doubt but that Thorndike meant that had *ideas* been present to guide the reactions that the latter would have succeeded within a few trials. Hobhouse would seem to agree with Thorndike that such a curve as the latter claimed to present was evidence of the absence of imagery. His criticism of Thorndike is to the effect that the former's curves are *not* gradual—"unless the slope of a church steeple is gradual."

The (apparently) common assumption of these writers has been questioned effectively both by Watson[21] and by Hicks and Carr.[22] The criticism of the latter authors is factual and is summed up as follows: "Our results indicate that any inference from such a general characteristic of a curve is not feasible, because we are dealing with a complex phenomenon due to several independently variable factors. Our results indicate that

[20] Thorndike, E. L. Animal Intelligence. *Psy. Rev. Mon. Supp.*, 1898, vol. 2, p. 45.

[21] Watson, J. B. Kinaesthetic and Organic Sensations. *Psy. Rev. Mon. Supp.*, 1907, vol. 8, pp. 23–4.

[22] Hicks, V. C. and Carr, H. A. Human Reactions in a Maze. *Jour. of Animal Behavior*, 1912, vol. 2, pp. 116–118.

the rational status of a group of animals can not be inferred from the slope of a curve in so far as this slope is dependent upon the number of trials or the relative rate of elimination. They indicate, moreover, that inferences as to intelligent status are legitimate in so far as the slope is determined by the factor of total values eliminated, but that the relation between the abruptness of slope and the degree of rational ability is just the inverse of that assumed by Thorndike and Hobhouse."

5. Memory

Arguments for the existence of ideas, have also been drawn from behavior purporting to be guided by memory—in the psychological sense. Let us use an example from Lloyd Morgan. The quotation of a few sentences will adequately represent his position when the Introduction to Comparative Psychology was written. "In the first place we may notice that the existence of memory is implied in the association of ideas; or rather in the occurrence of ideas at all." "If, therefore, animals have ideas at all—and if they have not we need not attempt to carry any further our investigations into zoölogical psychology—they must have memory, and there must be in them, as in us, some anatomical and physiological basis for what is popularly termed the retention of ideas." [23] By idea Morgan understands any centrally aroused conscious process. Of course Thorndike's results showed long ago that the presence of ideas in an animal requires vigorous proof rather than mere assumption. Now for a concrete example: "When I was at the cape I used to take my two dogs up the Devil's Peak, an outlying point of Table Mountain. There were several places at which it was necessary that I should lift them from ledge to ledge since they could not scramble up by themselves. After the first ascent they always remembered these places and waited patiently to be lifted up. On one of our first ascents one of them put up a young coney and they both gave chase. Subsequently, they always hurried on to this spot, and though they never saw another coney there, reiterated disappointment did not efface the memory of that first chase, or so it seemed. I think the last time I took them up must have been about three and a half years after the coney

[23] Morgan, Lloyd. Intro. to Comp. Psych. London, 1898, p. 117. Thus Morgan, as we noted for Hobhouse, seems to assume that ideas exist whether they function in behavior or not.

hunt: so long had the memory endured and the association remained uneffaced." [24]

This is a fairly well known example of the type of proof used by the "anecdotal psychologists." To some it may seem too trivial either for serious analysis or notice. But such a judgment is ill informed. We shall find similar arguments as late as Cole's paper on the Intelligence of Raccoons. The obvious criticism of Morgan's illustrations is that they may be simply cases of sensory recognition of the commonest kind. A further word will be said in connection with the criticism of Cole's work.

6. Thorndike's Test

A great many of the experiments which Thorndike presents in his recent book on Animal Intelligence[25] involve more or less intimately the question of the existence of images. However, I shall limit my analysis to the case in which the problem of the fact of images is most crucially attacked. The case I choose is the famous one reported in the first monograph of Animal Intelligence. I shall term it the "hand-clapping test" with cats. Thorndike's own words are such an excellent example of scientific description that I shall quote them at length:[26] "The only logical way to go at this question and settle it is, I think, to find some associations the formation of which requires the presence of images, of ideas. You have to give an animal a chance to associate sense-impression A with sense-impression B and then to associate B with some act C so that the presence of B in the mind will lead to the performance of C. Presumably the representation of B, if present, will lead to C just as the sense-impression B did. Now, if the chance to associate B with A has been improved, you ought, when the animal is confronted with the sense-impression A, to get a revival of B and so the act C. Such a result would, if all chance to associate C with A had been eliminated, demonstrate the presence of representtions and their associations. I performed such an experiment in a form modified so as to make it practicable with my animals and resources. Unfortunately, this modification spoils the crucial nature of the experiment and robs it of much of its authority. The experiment was as follows:

[24] *Ibid*, p. 118.
[25] Thorndike, E. L. Animal Intelligence. New York, 1911.
[26] *Ibid*, pp. 110-112.

"A cat was in the big box where they were kept (see p. 90) very hungry. As I had been for a long time the source of all food, the cats had grown to watch me very carefully. I sat during the experiment, about eight feet from the box, and would at intervals of two minutes clap my hands four times and say, 'I must feed those cats.' Of course the cat would at first feel no impulse except perhaps to watch me more closely when this signal was given. After ten seconds had elapsed I would take a piece of fish, go up to the cage and hold it through the wire netting, three feet from the floor. The cat would then, of course, feel the impulse to climb up the front of the cage. In fact, experience had previously established the habit of climbing up whenever I moved toward the cage, so that in the experiment the cat did not ordinarily wait until I arrived there with the fish. In this experiment

A=The sense-impression of my movements and voice when giving the signal

B=The sense-impression of my movements in taking fish, rising, walking to box, etc.

C=The act of climbing up, with the impulse leading thereunto.

"The question was whether after a while A would remind the cat of B, and cause him to do C before he got the *sense-impression* of B, that is, before the ten seconds were up. If A leads to C through a memory of B, animals surely *can* have association of ideas proper, and probably often *do*. Now, as a fact, after from thirty to sixty trials, the cat does peform C immediately on being confronted by A or some seconds later, at all events before B is presented. And it is my present opinion that their action is to be explained by the presence, through association, of the idea B. But it is not impossible that A was associated *directly* with the impulse to C, although that impulse was removed from it by ten seconds of time. Such an association is, it seems to me, highly improbable, unless the neurosis of A, and with it the psychosis, continues until the impulse to C appears But if it does so continue during the ten seconds, and thus get directly linked to C, we have exactly a representation, an image, a memory, in the mind for eight of those ten seconds. It does not help the deniers of images to substitute

an image of A for an image of B. Yet, unless they do this, they have to suppose that A comes and goes, and that after ten seconds C comes, and, passing over the intervening B blank, willfully chooses out A and associates itself with it. There are some other considerations regarding the behavior of the cats from the time the signal was given till they climbed up, which may be omitted in the hope that it will soon be possible to perform a decisive experiment. If an observer can make sure of the animal's attention to a sequence A-B, where B does not arouse any impulse to act, and then later get the animal to associate B with C, leaving A out this time, he may then, if A, when presented anew, arouses C, bid the deniers of representations to forever hold their peace."

First as to the data obtained, Thorndike's results indicate only the magnitude of the interval between two stimuli which association can bridge. Using his symbols, B and C have been associated before this experiment was begun. The hand clapping, A, now precedes B by ten seconds. At the end of from thirty to sixty trials, the cat climbs up at A rather than waiting until B appears. Now must we assume either that the "A" neurosis, and hence the "A" psychosis, persists or that A has resulted in the central arousal of B? Not at all. There is good evidence to show that association in animals can bridge an interval of ten seconds and more. Nearly all behavior experiments cover at least ten seconds from the beginning of the test to the acquisition of food. Yet it is necessary that the first and the last of the test be associated in order to provide a motive for the complete reaction. In no case—Thorndike's not excepted—is the ten seconds a sheer gap. (Thorndike did not describe the behavior of the cats during the interval, although he did refer to it.) The animal is reacting during the interval. Motor attitudes at least are present to fill the gap. An animal as high in the scale as a cat could certainly form this simple association between a sound and a single reaction within sixty trials. Moreover, it is to be remembered that only two cats succeeded within this time. Two others were tested for one hundred and thirty-five trials, but uniform reactions were not secured. The situation would have been quite different had there been two or more signals, "A's," and as many different

responses. But even under such conditions, one would still be studying the association of stimulus and response. This would remain true as long as it was the *feeding* and not the animal's *reactions* that were delayed. On the other hand, if both the feeding and the reactions were delayed after the stimulus had been given, then *if there were such states as ideas* one would expect them to function here. Furthermore the need for ideas would increase with the number of different stimuli and reactions.

Just a word now concerning Thorndike's formula for the study of imagery. The fact that he himself was unable to carry out experiments in conformity with it, and that none have been carried out since his attempt, does not speak very well for the formula. I must confess my own failure as yet to perfect a technique by which the formula might be applied to animals. In order to associate A and B, it will be necessary that they be followed by a reaction x. A, B and x are now associated. B and C may now be linked together through a second reaction y. Even granting the ideational character of reactions carried out according to the formula, one would have to know the following facts concerning the above test: (1) Did the animal discriminate between A and B, between B and C, and between A and C? If the first and second discriminations were not made, A and B or C would have been directly associated through x or y. If A and C were not discriminated, the association B-C would have been useless. Difficulties such as these lead me to believe that the goal aimed at is unattainable. In fact, Thorndike states that the formula is valid only when B arouses no impulse to activity; this is the essential weakness of the formula, for one can never be certain of the absence of these intervening mediating motor tendencies. In fact their presence is extremely probable. It cannot be too often reiterated that structural psychology has no place in the study of animal behavior. One must speak in terms of function. It is impossible to tell whether an *image* is present or not. The most that one can ever say is that some process other than overt motor activity is present which functions as an image might in human consciousness. This amounts to an acceptance of Hobhouse's statement (although I do not feel that he always limits himself to this) that the ideas we deal with are

"practical ideas," understanding by this a *function* which does for animals that which practical ideas do for human behavior.[27]

7. Learning by Being "Put Through"

Perhaps the most important and best known piece of work on the presence of imagery in animals is that by Cole on the Intelligence of Raccoons. Let us consider the evidence which Cole presents. The argument derived from an animal's learning a problem by being "put through" may be analyzed first. Cole writes in particular reference to Thorndike, saying "It would seem that nine-tenths of the experimental evidence for the absence of ideas in dogs and cats comes from their inability to learn from being put through."[28] Again, "If inability thus to learn is evidence against the presence of ideas, then ability to do so should be equally strong evidence for it."[28] In an earlier paper,[29] I have discussed some aspects of this problem in the light of experiments carried out upon the white rat. This phase of the question need not be gone into more fully here. Whether or not it seems probable, from a speculative point of view, that an animal must use "free impulses" or images in order to learn from being "put through," we need not consider. My contention in the paper mentioned is that the data so far at hand do not warrant conclusions as to the presence of imagery. Furthermore I indicated that the behavior could be explained better in other terms. Now with reference to that type of experiment in which the problem learned is that of working latches rather than climbing into boxes, I believe the data presented by Cole are conclusive, as far as the facts are concerned. Some raccoons at least appear to learn by being "put through." Whether all raccoons would do so is, of course, quite another matter. But given the fact, it does not follow that one must necessarily interpret it as an evidence of the presence of images. Cole seems to have carried over this interpretation rather uncritically from Thorndike. The entire process can be adequately stated in sensational terms. Certain

[27] Hobhouse, L. T. Mind in Evolution. New York, 1901, p. 283. That Hobhouse does not limit himself strictly to this may be seen by reading the first few sentences on p. 284.

[28] Cole, L. W. Concerning the Intelligence of Raccoons. *Jour. of Comp. Neur. and Psych.*, vol. 17, 1907, p. 249.

[29] Hunter, W. S. A Note on the Behavior of the White Rat. *Jour. of Animal Behavior*, vol. 2, 1912.

stimuli, x, y, and z, e.g., are made prominent by directing the animal's attention to them. These stimuli occur in connection with one another and with certain movements, kinaesthetically reported, and are followed by the acquisition of food. What could be more natural, then, than that the cognizance of the stimuli should set off the associated movements? The behavior noted by Cole bears out this contention. All four raccoons, both the pair put through and the pair not put through, solved the problem of escaping from the box (No. 4) by working the fastenings at one trial in one manner and at another in another fashion.[30] This need mean no more than that several responses, as opposed to a fixed series, might follow upon certain stimuli. This would be a higher type of behavior, to be sure, than where only one response was given, but it would not therefore involve a new type of conscious process. "If the act which he (the raccoon) is put through is the one which will remain the easiest and the most convenient for him throughout the tests, irrespective of his position in the box, he will never vary from it. If not, he will employ your act when his position makes it convenient and he is looking at the latch you began with."[31] We are not told whether the raccoon learned which was the easiest way by trial and error or not. But it is to be inferred from the behavior of raccoon No. 2 that such was the case.[32] The behavior thus described is interesting, but entirely inadequate as far as the presence of imagery is concerned. It may well be that "animals which, so far as we know at present, are utterly unable to learn save by innervating their own muscles" are devoid of ideas, without its following that if this type of learning is present, the animal possesses imagery. Hence assuming the facts that Thorndike and Cole present to be unquestionable, it need only follow that the raccoon exhibits more complex sensori-motor behavior than the dog and the cat, and not that it shows a new type of behavior, i.e., a type of behavior involving the functional presence of a representative factor.

Cole adduces further evidence for the presence of imagery.[33] These may be listed as he himself presented them: (a) Recognition of objects; (b) forgetting; (c) variability; (d) associa-

[30] Op. cit., p. 243.
[31] Op. cit., pp. 245–6.
[32] Op. cit., p. 246.
[33] Op. cit., p. 251, ff.

tion by similarity; (e) reluctance and expectancy; (f) varying means to the same end, and (g) reactions to colors presented in a temporal series. (c was treated under 7, above.) Most of these arguments can be dismissed summarily. It should never be forgotten that although almost any type of behavior *may* involve imagery, the comparative psychologist is seeking for behavior whose explanation *requires* the assumption of such a function, even under the law of parsimony. Is the determining sensory stimulus present or absent at the moment of response? If it is present, why *should* the animal use a representative factor? These are the questions that every investigation as to the presence of images in animal consciousness must face.

8. Recognition

It seems to me extremely obvious that the fact of the recognition of a food bottle need not be interpreted as presupposing imagery. In fact it is hard to understand how imagery would function in such a situation! Recognition of this type does not necessarily imply memory or the dating of an experience in one's past. On this basis all animals must be granted the possession of images.

9. Rate of Forgetting

The fact that some of the raccoons forgot the solution of the boxes after an interval of three days does indeed indicate, as Cole claims, that automatisms had not been set up. But one must not infer therefore that images were involved. There is no factual support for the assumption that imaginal forgetting is more rapid than sensory. The same is true of the variable nature of the raccoons' behavior discussed above. This very probably indicates a high order of adjustive ability on the sensori-motor level, but not necessarily an "imaginative" adjustment. Mere variability of response is present in all animals. Do all animals, then, possess images?

10. Association by Similarity

"Association by similarity," or the fact that a raccoon will attack a certain fastening even after its location in the box has been changed, when contrasted with the activity of cats[34]

[34] Cole, L. W. Op. cit., p. 253.

and rats[35] that attack the old position rather than the old fastening, proves for raccoons only the superior importance of "objects,"—or the form, size and quality aspects of the stimulus,—over kinaesthetic space controls, i.e., the position aspects of the stimulus. In addition, it should be noted that Davis in his study of raccoons[36] obtained data of the opposite nature. His animals would claw at the spot where the fastening had been. But aside from all this, I see no reason why "association by similarity" should not be purely perceptual and hence be simply a type of recognition. As a matter of fact, all animals have responses (instinctive reactions for example) that are applied to classes of objects. Someone also has well said that animals, in cases like the present ones, simply *fail to see the difference* between two objects and hence react as though the two were the same.

11. Reluctancy and Expectancy

Is the "reluctancy" or the "expectancy" which appears to be manifested in an animal's behavior toward a difficult and an easy box respectively to be taken as evidence of the presence of imagery? Cole, e.g., says "no one who saw the animals resist being put into a box failed to credit them with a rather distinct memory of the difficulty of escape."[36a] By "distinct memory" Cole undoubtedly means an imaginal process. But *do* the facts prove this? Is the case not perfectly amenable to a "stimulus and response" explanation? The raccoon has associated a certain box with a certain displeasure until the presentation of the box arouses immediately the negative reaction. The raccoon *may* have had images of his previous experiences, but the facts do not prove it. One does not *need* images to explain this behavior any more than to explain a child's refusal to take a second dose of bitter medicine.

12. Varying Means to the Same End

The data presented by Cole under the heading "varying means to the same end" are just as inconclusive as that presented above, although they are more suggestive. We have here the activity of four raccoons directed toward entering a

[35] Richardson, Florence. A Study of the Sensory Control in the Rat. *Psych. Rev. Mon. Supp.*, vol. 12, 1909, p. 38, e.g.

[36] Davis, H. B. The Raccoon: a Study in Animal Intelligence. *Amer. Jour. Psych.*, vol. 18, 1907, p. 470.

[36a] Op. cit., p. 253.

box containing an apple. The raccoons were accustomed to reaching through a hole in the top of the box in order to procure the fruit. When a block with a steeple in it was placed in the hole, one raccoon immediately clawed out the block and ate the apple. "She seemed to work as if actuated by a thought of the apple in the box. It was not done by random clawing, nor could she smell or otherwise perceive the piece of the apple in the box."[37] We are not informed *why* the animal could not smell the apple. The fact that the fruit odor was in the room will not suffice. But even assuming this to have been controlled, we need not attribute an image of the apple to the animal. Habit got the raccoon to the hole and started her paw, and the contact (?) of the staple initiated the claw reflex. This plus the pleasurable results associated with the box are sufficient to explain the activity. In a slightly different experiment, the animals crawled through a hole in the top of the box in order to procure the apple. I now quote Cole: "The box had no bottom and instead of resting directly on the floor it rested on a row of bricks. Removing one of these made an opening under the lower edge of the box through which the raccoon might crawl. The opening in the top was now closed and nailed fast. No. 1 was freed, went to the top of the box and tried to claw out the block. He then walked about the room, then tried the block again. He then went to the opening made by removing the brick, stopped a moment, then crawled in."[38] To argue that this means *image* of apple is certainly naive, at least. Could the raccoon not sense the apple when its nose was within a foot (see description of box 18, op. cit., p. 215) of it? Again where the animal climbed up and over a roll of poultry wire in order to descend into the box, the possibility of the presence of imagery is only suggested, not proved. The opening into the box, as well as the odor of the food, was there impelling the raccoon to approach. What more natural, then, than that the animals should climb the wire and thus reach the food. Such behavior is what would be expected of raccoons that lived in a wire cage. The case of raccoon No. 4 is somewhat different. With the box which possessed two openings, he went directly into the lower of the two at the

[37] Op. cit., pp. 254–5.
[38] Op. cit., p. 255.

first trial. What influence his starting point in approaching the box had upon his success, we are not told.

Logically, the position taken by Cole in his illustrations would require him to argue that ideas are present wherever "motor excess" in learning occurs. There a sensorially reported situation calls out in succession the animal's repertoire of instinctive and habitual acts. This is a variation of means toward the attainment of an end, and is on a par with the "variability of response" argument discussed above.

13. Reactions to a Temporal Series of Colors

It is only fair to Cole to note that his main emphasis does not rest upon the above data, but upon a series of tests that he made with colors presented in a temporal series. Three colors, white, blue and red, were placed upon three levers which in turn were secured by a single pivot on the back side of a board one foot high. When the colors were presented in the order W, B, R, the animal was to secure food by climbing upon a box. When R, R, R was given, no reaction should be made. Now since the terminal stimulus was identical in each case, Cole argues, the only means by which the animals could react discriminatingly is by remembering what colors of the series had preceded. The fact that the raccoons clawed up the cards from behind the screen, reacting only when the proper one appeared, was also used as evidence of images. Believing that these tests were almost absolutely uncontrolled and that the interpretations were invalid as far as the data presented were concerned, I set two graduate students, F. M. Gregg and C. A. McPheeters at work upon this problem. Their purpose was: (1) to duplicate as nearly as possible Cole's results under adequately controlled conditions, and (2) having set up the discrimination, to determine and not to assume its basis. Their results will soon appear in the Journal of Animal Behavior under the title Some Reactions of Raccoons to a Temporal Series of Stimuli. I shall only note here that they found: (1) that discrimination was not based upon the cards—in fact the discrimination was not even visual, and (2) that practically the entire discrimination was made on the basis of the first lever and not on the basis of all levers as Cole assumed. (Since all levers were influential, in Cole's opinion, it had been necessary to

assume that the first two were represented imaginally when the last was presented.)

The criticisms on Cole's entire work as outlined above reduce to these: (1) The facts are either inconclusive or irrelevant. And (2), there is no evidence of adequate controls. On the positive side, the work suggests that the raccoon is more intelligent than the dog and cat, but it does not determine wherein this superiority lies.

14. Washburn's Cat on the Stairway

There is one other type of behavior that deserves mention. Again, it was not and need not be interpreted as necessarily involving the presence of imagery. The illustration follows: "A cat, indeed, once observed by the writer, did behave as a human being would do to whom any idea had occurred, when, on coming into the house for the first time after she had moved her kittens from an upper story to the ground floor, she started upstairs to the old nest, stopped halfway up, turned and ran down to the new one. But errors of interpretation are possible at every turn of such observations."[39]

This is an excellent illustration of the type of argument that would use "hesitation" and "wavering" as an evidence for the presence of ideas. It is a mode of behavior that is found almost everywhere in animal studies. A rat, e.g., hesitates at a division point of the maze and finally selects the right pathway, or it runs *half* the length of a blind alley and then turns back. Was it guided by an ideational representation of the movements to be made and their consequences? Not necessarily. Accidental stimuli may have initiated the new reaction and any conflict present may have been resolved on a purely sensory-motor level. The experimental technique for the control of such reactions will be discussed below (see p. 74).

There is very little that needs to be said in the way of a summary of this historical review. All of the arguments for the presence of imagery in animals that we have examined have been found inconclusive. It is not that the various types of behavior may not have involved a representative factor. The point is that this possibility is nowhere proved necessary. The fault does not lie in the exhaustiveness of the data. The vari-

[39] Washburn, M. F. The Animal Mind. New York, 1909, p. 272.

ous methods have been made to yield ample returns for this purpose. The crux of the matter is that the methods themselves are inadequate for the solution of the problem. Let me re-emphasize the fact that if comparative psychology is to postulate a representative factor, it is necessary that the stimulus represented be absent at the moment of response. If it is not absent, the reaction may be stated in sensori-motor terms. But in order to know that the stimulus is absent, it is first necessary to determine carefully what the stimulus is. None of the methods reviewed, I believe, meet these requirements. Whether the tests presented in this monograph do or not, the reader himself may judge.

III. NOTES ON THE ANIMALS AND CHILDREN TESTED

Four classes of re-agents were used in the experiments whose description is to follow: white rats, dogs, raccoons (Procyon lotor) and children. A few words descriptive of these subjects will not be amiss.

1. Rats

Twenty-two rats were used during the entire course of the experiments. Five of these were normal adults and were used only in preliminary tests in which the purpose was the perfection of a method. The remaining seventeen (normal) were all started in the experiments when approximately four weeks old. All were vigorous, healthy animals whose records may stand as typical.

2. Dogs

The two dogs tested were mongrels in whom the rat terrier strain was dominant. They were very bright and intelligent looking, very active, playful and affectionate,—indeed they seemed to possess all the qualities that are attributed to intelligent dogs in countless anecdotes. This was the unanimous testimony of many observers. The two dogs, Blackie and Brownie, both females of the same litter, were secured from an animal dealer when they were small puppies and were started on the preliminary tests at about the age of five months. They were usually kept in a kennel out of doors and remained in excellent condition during the experiments. Of the two, Brownie was the more aggressive and, to the ordinary observer, appeared possibly the more intelligent.

3. Raccoons

Four raccoons, two males—Bob and Jack—and two females —Betty and Jill, were tested. Bob and Betty had been pets and were secured from their owner when about five or six months old. Jack and Jill were caught in the woods when about two and a half months old. Preliminary experiments were started almost immediately with all four. The raccoons were and remained in perfect health throughout the experiments. The only physical defects possessed by any of them were the cataracts which developed in Bob's eyes about a month after his purchase. Although this interfered with his accurate vision of objects and resulted in his colliding frequently with them, he was able to distinguish such changes in brightness as were necessary in the experiments. This fact was demonstrated conclusively by many control tests which will be described later. Of the four, Betty was the quietest and most timid. She was the least promising subject among them. Further facts about these animals are given in the appendix.

4. Children

Five children were used in the course of the present tests: two boys, Hd and L, and three girls, F, M, and H. H, Hd and L were each approximately six years old. M was about eight years old; and F, about two and a half. Hd and L were in kindergarten work, and M and H were in the graded schools. The indications were that they were children both of normal ability and of normal intellectual advancement for their ages. F was a bright little girl and made an excellent subject. All of the children were more or less timid at first; but this was overcome, in all save possibly H's case, before tests were begun. Particular pains were taken with F. The experimenter was in her company a great deal, and by the beginning of the tests was a gladly accepted play-fellow.

IV. APPARATUS AND GENERAL METHOD

The plan of box A is presented in Fig. 1. (This box was used for the raccoons.) The box is made of $\frac{1}{4}''$ boards and is $2\frac{1}{2}'$ high with doors $7\frac{1}{4}''$ wide and $13''$ high. The light stimulus came from 3 c.p. 8 volt miniature carbon lamps, so wired that they might be switched on one at a time. The current was obtained

from a 220 volt lighting circuit and was passed through a lamp rheostat before reaching the discrimination box. The release box R was raised by means of a cord passed over a pulley in the ceiling and back to the experimenter at E. The first release box had glass over the top and sides. The right and left faces of the box were 12″ x 15½″. The front was 7½″ x 15½″. With this release box the distances to the entrances of the three light boxes (L) were unequal. Those at the sides were each 19¾″, while the distance straight in front was 20½″. These various inequal-

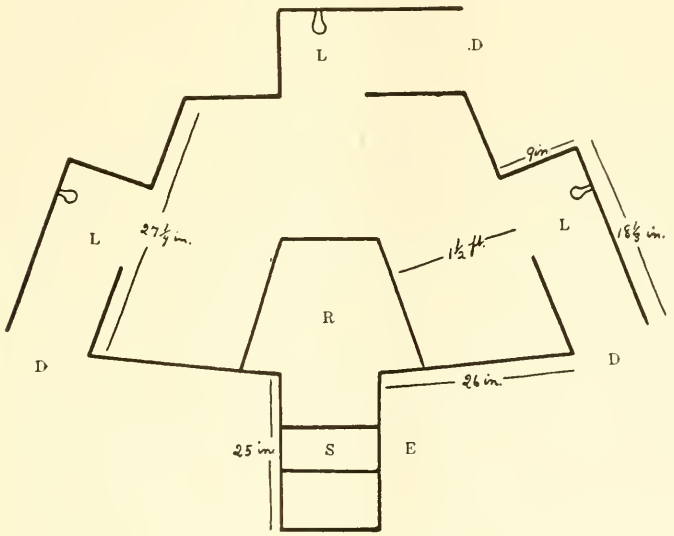

FIGURE 1. Ground plan of Box A

ities were due to two causes: (1) The box had been planned originally for a different type of test and was only later arranged for the present experiment. (2) The release box, being covered with glass would have been too heavy to handle had it been made larger. During the course of the experiments, another release was made. This one was covered with wire of ½″ mesh. Its sides were 14½″ x 15½″; and its front was 9½″ x 15½″. The distances to the light boxes were now equal and of the dimensions indicated in Fig. 1.

Sliding doors were placed at the points marked D in the figure. They were controlled by strings which ran from them,

through screw-eyes on the top of the box, to the experimenter at E.

A 16 c.p. light was suspended about four feet from the floor over the center of the apparatus. Its intensity was diminished approximately by two-thirds by wrapping the bulb in cloth. The three light boxes were covered in order to prevent light from entering them from above. Part of the entrance box leading to the release, R, was covered by the switch-board, S, and the remainder by paste board. This prevented the animals from watching the experimenter.

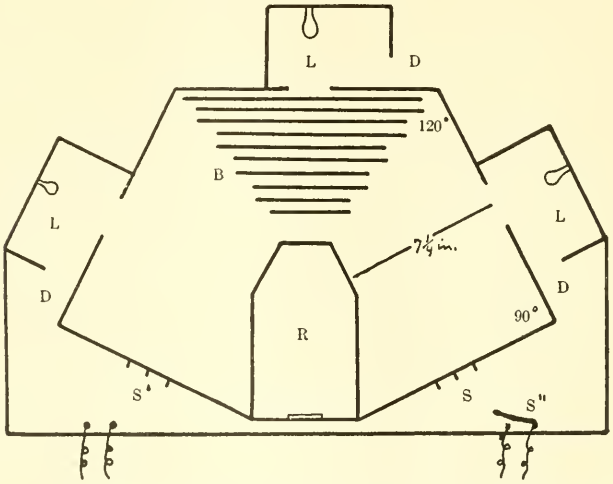

FIGURE 2. Ground plan of Box C. D, exit door.

Box B was, in principle, like Box A. It was used in testing the dogs. Its dimensions differed from those of A. Instead of being 90 degrees, the extreme lateral angles were 75 degrees. The angles on each side of the middle light box were about 145 degrees as opposed to 127 for Box A. Box B was only 2′ high, and the length of its entrance box was 1′ 4″. The release box was covered with wire, and each face was 1′ x 10″. The distances from the release boxes to the light boxes were each 1½′.

Fig. 2 is a ground plan of Box C. This box was used for rats. In addition to the data there given, the following points should be noted: The release box, R, was fastened by hinges

so that when it was raised the three faces cleared the floor practically the same distance. The faces and the front half of the top of R are covered with glass; the rest is of wood. The doors leading into the light boxes are 4″ x 3″. Those leading out of the light boxes, called exit doors here, are 3″ x 2½″. The switches at S′ are for the lights which are of the same intensity and wiring arrangement as those in the two preceding boxes. The switches at S turn the current into any or all of the paths from the release box to the light boxes. This current is obtained from a dry cell and is passed through the primary coil of a Porter inductorium. The strength of the current passing into the prob-

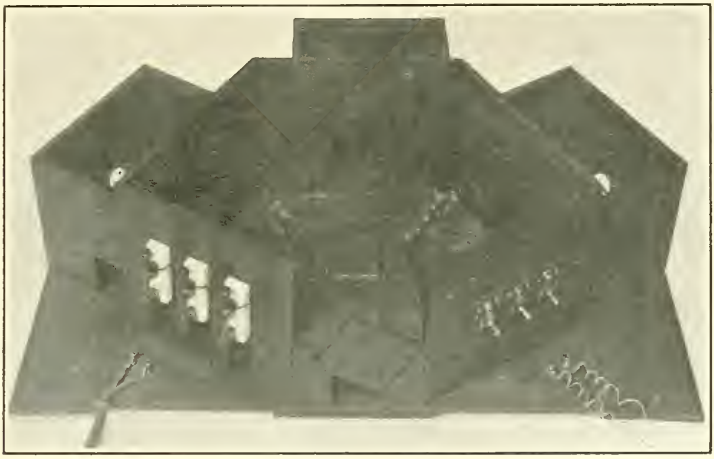

FIGURE 3. BOX C

lem box was regulated so that the animals never became frightened by severe shocks. The current was never strong enough to cause the animals to squeak and only rarely did they attempt to jump over the strips. The brass strips from which the shocks were obtained (only one group, marked B, is shown in this figure), were very thin and lay flat on the floor of the box. Before being tacked down, they were given an acid bath which destroyed their lustre but left their conductivity practically unaffected for my purpose.

Fig. 3 should give a clear presentation of the essentials of this box as well as of the others used in the course of this research.

Box D, also used for rats, is similar to Box C in all save two respects: (1) It was not wired for punishment. (2) The doors leading from the light boxes could be closed with wooden slides. The use of these slides was discontinued shortly after the experiments began. Pieces of wire mesh were then used. These admitted the light and thus offered less opportunity for the animals to tell which box was open and which was closed.

The apparatus (Fig. 4) which was used with children was constructed on the same principle as that described above for the other problem boxes. Three boards, each one foot square,

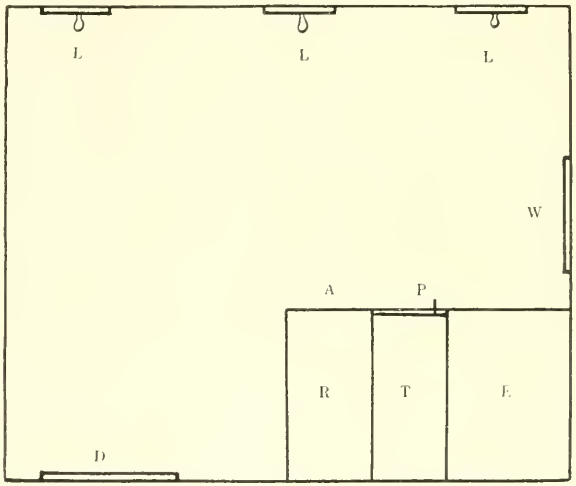

FIGURE 4. Ground plan of apparatus used with children. D, door; W, window

were placed against the wall of a room 12′ x 14′. The middle square was seven feet distant from the release box, R. On each of these boards was mounted: (1) A 4 c.p. miniature electric light, L; and (2) just below the light, a push button. All three boards were painted black and were exact duplicates the one of the other. The front, A, of the release box was a lever which could be raised by a handle at P. The experimenter usually sat at E. T is a table which held: (1) The candy used as a reward in the tests; (2) the switches regulating the lights and the buzzer; and (3) the buzzer. C is a curtain which hid the experimenter from the subject's view when the latter had

left the release box. At first this curtain was also continued between R and T. Subsequently, this was found to be both inconvenient and unnecessary and its use was discontinued. The apparatus was wired so that any light could be turned on at will and so that any push button could be connected with the buzzer. Moving the switches was done without the subject's knowledge. The light was always turned on over the button that rang the buzzer. The child's problem was to find this button at the first trial when the light was on (in the learning series) and then (in the delayed reactions) after the light had been turned off for a certain interval of time.

The general method of experimentation was as follows: The animal to be tested was put in the release box, R, of problem Box C, for example. If, now, the lighted box is the one on the left, the exit doors of the others are closed and the switches at S are so set that if the experimenter close the switch at S″, the animal will receive a shock if it steps on either the strips leading to the box on the right or on those leading to the one in front. The light is then turned on in the left box. The animal is released after five seconds, the time being taken with a metronome. A careful, detailed record is kept of the direction in which the animal is oriented[40] when released and of just where it goes after being released. In the case of the animals used in Boxes A, B and C, they should go straight to the lighted box, out through the exit doors and back to the entrance of the release box where they are fed. The rats used in Box D were fed a small morsel of bread and milk at the exit doors of the lighted boxes. Theoretically the olfactory control was not so good here as where no food at all was given in the light boxes. Practically, there was no difference. The rat was given only a bite, so almost no food fell on the floor; all the boxes were used an equal number of times; and all were frequently washed out. Whatever odor was present was so distributed as to afford no appreciable basis for discrimination. The results obtained with these rats, when compared with those where the olfactory control was better, support this statement. In any event,

[40] Orientations are spoken of as right and wrong, irrespective of whether an animal may be said to depend on them as cues or not. When the orientation is "right," the animal is headed toward the proper box. Any other orientation is "wrong."

olfactory inequalities would persist after the light was turned out and would aid in delayed reactions only if associated with the light. More attention will be given to this possibility later in the discussion.

After the animal had been trained until it chose the lighted box almost perfectly, delays were begun. The light was turned off just as the animal reached the box. This was called the first stage of delay. At the second stage, the light was turned out when the animal was half way to the box. At the third stage, the light was turned out just as the experimenter *started* to raise the release box. Here there was a genuine delay, although a small one. The first two stages served primarily to adapt the animal emotionally to the sudden change from light to darkness. The rats and dogs usually ran so fast that their momentum was sufficient to carry them into the box when once they were started toward it. In any case they only needed to continue in the direction in which they were going. This, however, was not the case with the third stage. *The light was put out before the animal started.* Throughout these three stages, the animal was released promptly at the end of five seconds. From this stage on, where the animal was detained one or more seconds after the light was out before being released, it was obviously necessary to let the animal see the light before this was turned off. Occasionally, the interval thus required was more than five seconds. In these higher stages of delay, I always waited until I felt sure that the animal had seen the light, and then turned off the current while the animal was still oriented toward the source of light. Record was kept of any change in the orientation which an animal made after the light was turned off. How detailed these records were will be seen in the section on experimental results.

The delays were gradually increased in length until one was found at which the animal failed. They were then decreased until the animal was again making a high percentage of correct choices, when the intervals were again increased. An animal was thus tested twice for the limit of its ability to delay with the backgrounds surrounding the entrances to the light boxes all similar the one to the other. When this limit was found, the wall of the box about the entrance to *c* was covered with white cardboard; that about *b*, with a black; and that about

a,[41] with a medium gray. If the animal's limit of delay was no better or was worse with this arrangement than before, the animal was dropped from the experiment. If the limit were better, the different backgrounds were removed and the similar ones used again. The limit of delay with these was then re-determined. If this third limit were greater than the second, the effect of training could be evoked as an explanation of the fact. But if it were markedly less than the second, the only cause could be an association between the backgrounds and the lights. The significance of this type of association will be dwelt upon in detail later in this paper.

One more point in general method remains to be considered. This is the question of what percentage of correct choices shall be taken as sufficient to justify further increase in the interval of delay. With three discrimination boxes, pure chance would lead the animal to make $33\frac{1}{3}\%$ of correct reactions out of a long series of presentations. But series used in experimentation are very rarely long enough for chance to operate as theory demands. Besides there are various other influences which enter in to determine an animal's behavior above and beyond the influence of the stimulus proper to the test. One such influence is the position factor. Try as I might to eliminate this, most of the animals acquired at various times during the experiments more or less pronounced preferences for certain boxes. And these preferences varied from animal to animal. Sometimes they were so strong that the regular tests had to be stopped until the habit could be broken up. In the light of this, although on the whole each box was presented to an animal an equal number of times, in any one stage of the tests such an equality might not be present. Thus an equality of percentages among the various animals, in this case, would not mean that they knew the problem equally well. Again hesitancies and waverings must be noted in estimating how well the animal is grounded in his appointed task. Further statements concerning the value to be attached to the percentages appear later in the paper (pp. 44, 46 and at intervals thereafter.) Considerations such as these render it highly inadvisable to lay down a rigid standard as to the number of trials and the per-

[41] a, b, and c will be used in this paper to designate the right, middle and left light boxes respectively.

centage of correct reactions to be required of all animals. As will be seen in the following experiments some animals were advanced from stage to stage when but 70% of correct reactions were made. Others were detained on one stage until 85% or 95% were made. Sometimes an animal was given 5 or 10 trials, sometimes 150 or 200 trials on one stage of delay. One general fact, however, should be noted: The first set of rats, Bob and Betty during their first year's work, and H, L, M, and Hd of the children were rushed from stage to stage as fast as was at all feasible. The emphasis here lay upon what the subjects *could* do with their native equipment. With the other subjects used and with Bob and Betty during the second year's work, a larger number of trials were given on each stage of delay and the advance in the length of the periods was more gradual. The emphasis here was placed on what the subjects could *learn* to do.

V. EXPERIMENTAL RESULTS

1. Tests with Animals

A. Learning the Association. (a) Rats.—The method of experimentation used by Hough and Reed was retested with the result that the general method outlined above was adopted. These tests were made upon five normal adult rats, Nos. 18–22.

The regular experiments may be divided into two sets: (A) Those in which reward only was employed; and (B) those in which both punishment and reward were used. Nine rats, Nos. 1–9 were tested in the first set and eight, Nos. 10–17, in the second set. Of the rats in A, Nos. 1, 3 and 8 were in poor health, and their records will not be considered.

Table I gives the number of trials required by the rats of Set A to learn the association between the light and getting food. These rats were given 5 trials daily. All save No. 6 learned the association. Of the last 50 trials given this animal, only 40% were successful; while of the entire 800, 54% were correct.

TABLE I

Rat	No. of trials on learning
No. 2	176
No. 4	175
No. 5	505
No. 6	800
No. 7	361
No. 9	280

The rats of Set B were tested as follows: Nos. 10, 11, 14 and 17 were given 5 trials daily. Nos. 12, 13, 15 and 16 received 10 trials. All of these animals learned the association. The number of trials required for this learning is given in Table II. The

TABLE II

Five trials		Ten trials	
Rat	No. of trials on learning	Rat	No. of trials on learning
No. 10	165	No. 12	440
No. 11	160	No. 13	250
No. 14	200	No. 15	220
No. 17	175	No. 16	480

results here indicate: (1) That the use of 5 trials favors rapid learning more than does the use of 10 trials, and (2) that rats tested with 5 trials daily under conditions of reward and punishment will learn the association more nearly in the same length of time than will rats given the same number of trials but tested with reward only. The use of punishment, while it may not shorten the length of time required by an "intelligent" rat to learn an association does hasten the learning in the case of the "dullards" such as Nos. 5, 6, 7 and 9. Rats will put forth a maximum effort under punishment when they would not do so under other conditions. These results are in harmony with the work of Hoge and Stocking.[42]

That the influences on time of learning exercised by the number of trials and by punishment are not due to preferences for the light or for the dark room boxes is indicated by Table III which gives the number of times each rat chose the

TABLE III

Rat	No. 2	No. 4	No. 5	No. 6	No. 7	No. 9
No. of choices of light box	23	21	13	18	23	38
Rat	No. 10	No. 11	No. 14	No. 17		
No. of choices of light box	20	28	28	32		
Rat	No. 16	No. 15	No. 13	No. 12		
No. of choices of light box	24	30	21	27		

[42] Hoge, M. A. and Stocking, R. J. A Note on the Relative Value of Punishment and Reward as Motives. *Jour. of Animal Behavior*, 1912, vol. 2.

lighted box during the first 50 trials. The data given for the rats of each group were obtained under similar conditions (for our present purpose) and are strictly comparable. The only cases of marked preferences occur with rats Nos. 5 and 9. No. 5's preferences for the dark boxes will help to account for his slow learning; but it would seem that No. 9 should have learned very rapidly owing to its marked preference for the light box. With the exception of these two rats, all the animals were on an essential par as to preferences. Hence the differences noted in their learning periods must have been due to the different conditions under which they worked. Accidental individual variations are not the causes of these differences because of the number of rats used.

(b) Dogs.—The two dogs, Blackie and Brownie, were given 10 trials daily in Box B under conditions of reward only. The former animal was given 560 trials on the association. Of these, 408 trials or 72% were correct. Brownie was given 650 trials on learning the association. Of these, 396 trials or 60% were correct. Ninety-five per cent of the last 100 trials were successful. The relatively poor showing made by this dog was due to the acquisition of habits that interfered with the proper reaction. Twice during the learning series, extra tests had to be given in order to break up a discrimination now against the right and middle boxes, now against the left and middle boxes. In general, it may be said of both dogs that their rate of learning is no better than that of the rats. Indeed it is much worse than any of the rats tested with reward and punishment and 10 trials daily. The only rat that learned the reaction that did not do much better than the dogs was No. 5. I would suggest that the difference is due to the dogs' "helplessness" when deprived of cues from the experimenter. The following paragraph elaborates this point.

There are several interesting points relative to the dogs' behavior in the learning series that deserve mention. Indeed to a large extent, they are typical of their behavior throughout the entire experiment. When the dogs were first put into the problem box, instead of attempting to get out, they merely sat down and howled. They were out of sight of the experimenter, it will be remembered, and when placed in such conditions seemed quite helpless. They gradually overcame this timidity,

helplessness or lonesomeness but, as will be seen in the delayed reaction tests to be described below, they never revealed a high order of resourcefulness. We shall have further occasion to see that their behavior was almost, if not entirely, on a par with that of the rats. In learning the association, e.g., the dogs would go back and forth between some two boxes for many trials before investigating the third box. Often they would stop and look around in an apparent attempt to find the experimenter. This was never done by the rats and raccoons. Again, the brown dog fell into the most absurd habit of going out of the release box, turning entirely around to the left and then going to one of the boxes. This accomplished no end that I could determine, yet it was persisted in for some weeks.

(c) Raccoons.—Bob and Betty were each given 10 trials daily. Jack and Jill received 15. Reward only was used, although running into the wrong box and having to back out constituted no little punishment here as was also the case with the dogs. The experimenter was practically out of sight of the animals all of the time and absolutely so in certain control tests. It will be remembered that the raccoon tests were made in a room illumined by a single light suspended above the problem box. (See p. 24). This left the remainder of the room dark and thus helped conceal the experimenter.

Bob required 120 trials to learn the association. Of these, 93 trials or 77% were correct. Of the last 50 trials, 96% were successful. Betty was given 340 trials in the learning series. The work was not properly controlled for her until the last 120 of these trials. She formed the habit early of looking into the light boxes and seeing whether or not the wooden exit doors were closed. This difficulty was finally obviated by using coarse mesh wire for the doors. These could not be sensed until the animal practically touched them. Of Betty's last 50 trials on learning, only 79% were correct. These tests should have been continued longer. Jack was given 540 trials on learning. Of these, 428 trials or 79% were correct. Of the last 150 trials, 98% succeeded. Jill received 825 trials during the learning period. Of these, 631 trials or 76% were correct. Of the last 150 trials, 99% succeeded. (The experiment box had wire exit doors when Jack and Jill were tested.)

Although the nature of the learning process is not our specific problem, the differences in the total number of trials required for learning by Bob and the last two animals are so great as to invite comment. The conditions under which the records were obtained differ in the following points: (1) Jack and Jill were younger than Bob by at least a year. (2) Work was begun with them in July and with Bob in (the preceding) October. This brought him nearer the period of hibernation when his appetite would begin to fail. (3) Jack and Jill received more trials daily and also were tested for more days. Considering the fact that when the experiments ceased Jack and Jill were younger than the other two raccoons were when the latter started and yet had accomplished as much, I do not believe that age was an important factor in determining the reactions. However, the actual fact of an age difference remains. The second point should have little explanatory force, since the animals were always eager to work. As to the third point, one would expect this factor to work in a direction opposite to that which the results indicate. However, such was not true with the rats, and it may be that here also the lesser number of trials daily is more favorable to rapid learning. The fact of a possible difference of preference for light and dark remains to be considered. Of the first 50 trials, Bob made 56% correct. Of the first 45 trials, Jill made 22% correct. Jack made 48% successful responses out of the first 45 trials. Age differences, differences of brightness preference and differences in the number of daily trials are possible explanations for the varying lengths of the learning records. Innate (?) individual abilities must also be recognized. (Betty's results are not considered because of their unreliability at this point of the experiments.)

Summary.—The only comparative statement with reference to the learning periods of the different groups of animals that the facts warrant is this: The raccoons Jack and Jill and both dogs belong together in the class that learned most slowly. The second class with reference to speed of learning is composed of the rats. Bob is in a class by himself. He learned more rapidly than any of the other animals. Further comments upon this aspect of the learning process will be deferred to the section on children.

B. Maximal Intervals of Delay Attained. (a) Group differences as to maximal delay.—The only conditions of experimentation that varied from animal group to animal group were these: (1) Number of trials daily. Rats 2, 4, 5, 6, 7 and 9 (Set A) and rats 10, 11 ,14 and 17 (of Set B) were given 5 trials daily. Rats 12, 13, 15 and 16 (of Set B) received 10 trials. The two dogs were given 10 trials daily. Of the raccoons, Jack and Jill were each given 15 trials and Bob and Betty 10 trials daily. (2) The rats of Set B were tested with reward alone. As was mentioned above, the dogs and raccoons were very much discomfited by having to back out of the light boxes when a wrong choice had been made. This was even more true in our tests than in most discrimination work owing to the small size of the light boxes.

TABLE IV

	Rat	Maximal delay	Per cent of correct response
Set A	2	1 sec.	64
	4	1 sec.	52
	5	3rd stage	52
	6	did not learn association	
	7	3 secs.	56
	9	10 secs.	72
Set B	10	1 sec.	76
	11	1 sec.	64
	*12	1 sec.	72
	*13	4 secs.	88
	14	3 secs.	80
	*15	1 sec.	86
	*16	1 sec.	50
	17	1 sec.	37

Set A tested with reward.
Set B tested with reward and punishment.
Those rats that are marked with a * received 10 trials daily. All the others received 5.

Table IV gives the maximal delay attained by the rats. The percentages are computed on a basis of 25 trials. It will be seen from this that only four rats of the fourteen tested reached intervals greater than one second. The percentage of correct behavior for one of these (No. 7) was only 56. Rats Nos. 4, 11, 15, 16 and 17 were tested with a choice of two boxes as opposed to three. This was done only after the animals had

more or less completely broken down in their reactions to one box. Tests with two boxes were never made on any subject, unless the animal lost the cue to one box. It was this box that was dropped from the series. The maximum delays made by the rats under these conditions were increased. Nos. 11, 15 and 16 each made 5 secs. at from 76 to 90%. This must mean that it is easier to use two cues than three. Further comments will be added below (see p. 39).

Of the dogs, Blackie made 10 secs. at 76% for 30 trials. She then lost the cue to one of the boxes and was unable to react successfully to that one. When tested with two boxes she finally made a delay of 5 mins. for 5 trials at 80%; 86% had been made at 3 mins. for 30 trials. Brownie was tested only with three boxes. She made a delay of 1 sec. for 50 trials at 90% and 2 secs. for 70 trials at 68%. Both dogs were tested for eight months. The results should be representative of what dogs can do under the present conditions.

Of the raccoons, Jack made 14 secs. at 70% for 30 trials. Tested with two boxes, he finally made 85% at 20 secs. for 40 trials. He was tested for eight and one-half months. Betty made 82% at 7 secs. for 50 trials. Tested with two boxes, she reached 10 secs. at 86% for 30 trials. She was tested for one year and three weeks. Jill was tested only with three boxes. She made 3 secs. for 45 trials at 93%. The tests extended over a period of seven months. Bob made 90% for 10 trials at 15 secs. Tested with two boxes, he reached a delay of 25 secs. for 20 trials at 90%. Bob was tested steadily for a year and five months.

For fear that some critic would urge that the animals could not have delayed longer than they did even though the light had been constantly present, control tests were made as follows: The animals were taken at the stage when they had just broken down at some (for them) long interval of delay and were held in the release for one minute with the light on. When they were released, the light was still left on. The results show that the animals made a very high percentage of correct behavior. Their failure to make correct long delayed reactions must have been due, then, to their inability to use some cue by which to guide their reactions after such an interval and not to mere restlessness caused by the long confinement.

Appendix A contains tables that show all the regular tests given to the raccoons Bob and Jill and to rats 4 and 16. The data are there given in the order in which they were obtained. They are typical of the results of all the animals.

There is so much individual variation within the three groups of animals whose results have just been given that any exact correlation between length of delay and groups of animals is unwise. It is to be borne in mind moreover that it is not the length of delay but the methods of reaction after delay to which the greater importance attaches. This topic will be discussed later. To give some further idea, however, as to the relation between the groups Table V gives the longest and the shortest delay made by each class of animals. This table ignores the different conditions under which the delays were obtained. It presents the maxima and minima of the best reactions that the individual animals of a group were able to make under the present conditions.

TABLE V

Subjects	Min. delay	Max. delay
Rats	Either no learning or 3rd stage	10 secs.
Dogs	2 secs.	5 mins.
Raccoons	3 secs.	25 secs.

(b) Effect of size of release upon interval of delay.—The experiments so far described were all made with the small release described in the section on apparatus. This release confined the animal's activities to a small part of the apparatus. It was thought if a release was used which would give the animal the freedom of the whole interior of the box, that not only might the maximal interval of delay be increased, but the animal might reveal more clearly its method of solving the problem,—indeed it might even develop a new and higher type of behavior in response to the more complex situation. These latter possibilities, we shall consider below.

The new release that was used fitted just inside the openings into the light boxes. It was made of a continuous piece of wire netting, thus making possible the simultaneous presentation of all three boxes. It was now possible for the animal to go over to the door of the lighted box and wait there during the interval of delay. The light was always left on until the animal had reached a position immediately in front of the lighted box.

Preliminary tests insured an absence of emotional disturbances in the reactions.

The animals tested under these conditions were: Rats 13, 15, 16 and 17 (No. 17 was the only one tested with a choice of three boxes); both dogs (Brownie alone was tested with a choice of three boxes); raccoons Jack, Jill and Bob (Jill alone was tested with three boxes).

Table 6 gives a comparative statement of the animals' abilities with large and small release. Both records for each animal are for discriminations with the same number of boxes. It will be seen from this that rat No. 13, the dog Blackie and the raccoons Jack and Bob are the only ones that failed to delay longer with the large release. This fact can be readily explained in the case of Jack and Blackie. The experimenter was primarily interested in discovering whether any new mode of behavior would appear under these conditions. When this question was answered, tests were discontinued with all the animals although in the case of Jack and Blackie the interval of delay was being steadily increased. The increase in delays made by the other animals is to be correlated with the changed conditions of experimentation. This point will be amplified when the detailed behavior of the animals is discussed.

TABLE VI

Animal	Small release		Large release	
	Delay	Per cent of correct reactions	Delay	Per cent of correct reactions
Rat No. 13	4 secs.	88	3 secs.	95
Rat No. 15	1 sec.	86	6 secs.	86
Rat No. 16	1 sec.	50	9 secs.	82
Rat No. 17	7 secs.	68	1 sec.	37
Blackie	5 mins.	80	1 min.	80
Brownie	2 secs.	68	6 secs.	96
Jill	3 secs.	93	7 secs.	80
Jack	20 secs.	85	15 secs.	88
Bob	25 secs.	90	20 secs.	76

(c) Effect of backgrounds of different brightnesses upon the interval of delay.—When the apparatus for the animals was constructed, every effort was made to secure qualitative similarity in the three light boxes. The backgrounds surrounding the entrances to the light boxes were all of the same brightness. During the course of the tests, the attempt was made to deter-

mine whether the animals *could* form an association between the lights and some constant marked differences in the external environment. In order to test this, three backgrounds of widely differing degrees of brightness were used as described above in the section on method. The animals tested under these conditions were: Rats Nos. 2, 4, 5 and 7 of Set A and Nos. 10, 12 and 16 of Set B; the dog Brownie; and the raccoon Jill.

The results of this test were entirely negative. No case was found where the animal made use of the different backgrounds as cues for guiding its reactions. One or two of the animals increased their interval of delay under the new conditions. However, when the old conditions of similar backgrounds were replaced, the long intervals of delay still continued. This indicated that the improvement was due to practice.

(d) Effect of number of boxes upon delay.—As stated above in section (a), whenever an animal's reactions broke down upon a particular box, that box was dropped from the tests and the discrimination confined to the other two. The following animals were tested under these changed conditions: Rats 4, 11, 15 and 16; the dog Blackie; and the raccoons Jack, Bob and Betty. The results are given for convenience above under section (a). Here it may be noted that all the animals delayed longer with two than with three boxes. This can be readily explained on the basis of the complexity of the problem. The maximum delay for any animal is decided by the accuracy of its response. With two boxes, this accuracy was increased and hence the maximum delay recorded was greater.

(e) Effect of other conditions upon delay.—The results do not indicate certainly any effect of punishment or of the number of trials upon the length of the interval of delay.

The results of this section indicate that the following factors influence the maximal amount of delay: (1) Different groups of animals; (2) size of release, and (3) the number of light boxes used. The following factors do not influence the amounts of delay: (1) Punishment and reward; (2) number of trials daily, and (3) backgrounds of different brightnesses.

C. Methods of Reaction After Delay Used by the Animals.—There are three different methods of delay which might have appeared and in point of fact did appear in our delayed reaction experiments: (1) The animal may maintain an orientation

of all or part of its body during the interval of delay, i.e., it may keep its head or even its whole body pointing toward a certain box. (2) There would be the negative side of this, where the experimenter could detect no orientation cues used by the animal. In this case, no observable part of the animal's body would remain in a constant position. (3) The animals might rely upon position in the box for their cues, i.e., they might actually go nearer to one box than to the others and then wait to be released. (4) Any combination of these three methods might occur. The discussion of methods 1 and 2 (orientation and non-orientation cues) will be combined and will be followed by a consideration of method 3. The actual existence of method 4 will be considered as occasion demands.

(a) Orientation of whole or part of body.—In addition to what was given above in the section on method concerning orientation, it will be well to make such additional comment here as will indicate clearly the nature of the data secured. Great pains were taken to insure accuracy and consistency in the recording of orientations. Needless to say in such prolonged tests as the present ones, the experimenter soon becomes expert in deciding whether an animal's movements are to be interpreted as a change in orientation. Before an animal has been tested long, the experimenter can pick out a certain range of movement and call this the orientation toward a certain box. The animal (dogs excepted) was in constant motion, but so long as its activity was directed toward any one face of the release box, the orientation was recorded as unchanged. There would seem to be some chance for doubtful cases when the animal was pointed halfway between any two boxes. These cases were never counted as changes in orientation. Record was kept not only of the body position, but of whether *any* observable part of the animal remained in a constant position during the delay. Further note was taken of the gross amount of the loss of orientation—i.e., whether the animal turned clear around or not; and if not, then how far around—and of just which reactions were preceded by apparently identical orientations. The data were recorded quickly and easily by the use of symbols.

Every rat at the moment of release, went in the direction of his bodily orientation in 99 cases out of 100. (At times this

was not true because of position habits formed by the animal.) (For the present purposes, it makes no difference whether the reaction was correct or not.) The data supporting this statement are so overwhelming that they need not be given here in detail. The rat, when put into the release box during the delayed reactions, oriented *immediately* to the light with its entire body and began a series of attacks on that side of the box in an effort to get out. This attempt was kept up until the animal was released, whereupon it went to the box that was straight in front. Experiment served only to lengthen the period during which they would attack any one side of the box. These statements hold true for all rats.

Both dogs were dependent upon orientation for the guidance of their successful reactions. They only differed from one another in the length of time during which they could maintain a certain orientation. The dogs differed from the rats in that the determining cue was the direction of the *head* rather than of the *body*. For the sake of concrete material illustrative of this type of reaction, I shall give a summary of typical reactions made by Blackie. Of 770 trials, given during a period of two months, on delays less than 3 secs. long, 141 were unsuccessful. On 116 of the 141, the dog had the wrong orientation at the moment of release and followed it. On the remaining 25 reactions, the dog failed to follow its orientation and was wrong. On 8 trials the dog had the wrong orientation at the moment of release, i.e., was not headed toward the proper box, and yet reacted correctly. However, only 3 of these trials were with a 2 secs. delay and may have been due to chance. The remaining 5 trials were at the second stage of delay where the light was on until the animal was halfway to it. Obviously, these 5 reactions signify no great ability. These results indicate that just to the extent that the dog was able to hold the proper orientation during the delay, just to that extent it was capable of reacting correctly.

Let us take another typical set of results from the same dog obtained on delays from 15 secs. to 5 mins. extending over 30 days. Two hundred and eighty-five trials in all were given of which 37 were incorrect. In all 37 trials, the dog had the wrong orientation and followed it. Only once did she have the wrong orientation and react correctly. When Blackie entered the

release box, she would turn clear around and then, before the final breakdown came, would lay down facing the light. Occasionally she would be distracted by some accidental noise inside or outside the laboratory. In the great majority of these cases, she only turned her head. A few times she got up and turned clear around. But in any event, if she did not recover the proper orientation, the reaction failed. If she could have lost the orientation either completely, or almost so, and then have returned to it and have reacted correctly, the fact would have been strong evidence for believing that the dog recognized the proper orientation when it was reinstated. But taking the last 125 trials as typical, and classifying the instances where orientation was lost, recovered and a correct reaction made, it is found that 33 times the orientation was changed but slightly and three times completely changed.—By a slight change is meant that the dog turned her head and not her body. By a complete change is meant that the animal turned completely around.—Of the three cases of this latter behavior, one was very probably due to chance. The other two occurred on the second day of the 5 min. delays when the final breakdown was beginning. Moreover, the orientation that was lost and recovered,—in one case at the end of 36 secs.; in the other at the end of 4½ mins.,—was the orientation toward the box at the right. The reaction is thus not significant of some higher process, for on the following day the box at the right was the only one to which the animal responded. The recovered orientation, therefore, most probably indicates solely the growth of the habit that, on the following day, resulted in the complete disintegration of the reaction.

It is interesting in this connection to trace the change that occurred in Blackie's behavior from the beginning to the end of the experiments. It has already been noted how "helpless" and inactive both dogs were when the tests were started. After the work had progressed for several weeks and Blackie had become quiet and attentive, she would stand in the release box on all four feet and occasionally paw the wire in the direction of the light. A little later, she sat on her haunches during the retention in the release, but still clawed at the wire in a very calm manner. Toward the last of the tests described above, when the delays were growing rapidly longer, she lay flat down

on the floor with her head pointed, usually, toward the light. But she did not lie quietly. The long delay seemed very trying. Blackie would whine, wiggle her body and pat the floor with her fore paws in a fever of impatience,—yet never change the alignment of her body. Many times the dog held her orientation almost until the last second of delay and then if, when the release came, her head was (apparently) not more than half an inch to the right of its earlier position, she went in that direction and consequently went to the wrong box. Surely this is weighty evidence against the functional presence of any higher processes. Everything points to the conclusion that Blackie's reactions were determined by the orientation of her head at the moment of release.

It has just been indicated that the maintenance of an orienation either of all or a part of the body was necessary in the case of the rats and dogs, if their reactions were to succeed. Such was not the case with the raccoons. Each of these animals could react successfully when the wrong orientation was held at the moment of release and when, so far as the experimenter could detect, no part of the animal's body remained constant during the interval of delay. The evidence in support of this generalization is perfectly conclusive. It is only possible—and necessary—to present typical cases here. I use Jack as an illustration, although he did not delay as long as Bob.

In Jack's first 800 trials of delays, 77 were wrong. In 7 of these 77, the raccoon had the wrong bodily orientation, but did not follow it. On 12 trials, he had the right orientation, but did not follow it. On 58 trials, he had the wrong orientation and followed it. Of the 723 correct reactions, 167 were made starting with wrong orientations.

In the following 1066 trials, 149 reactions were incorrect. This group of trials extends from a period of 8 sec. delays at which 75% was made through a series at 11 secs. where 91% was made. Forty-one errors were made when the wrong orientation was held but was not followed. Thirty-seven were made when the right orientation was held. Seventy-one were made when the wrong orientation was held and followed. Out of the 917 correct trials, Jack reacted 309 times correctly when his orientation was wrong.

It is interesting to note that almost one-fourth of the last

mentioned errors were made despite the fact that the proper orientation was being held. This is a type of reaction that almost never occurred with the rats and the dogs. It would seem to indicate that this raccoon is less dependent on gross motor attitudes than the other animals. But the most significant behavior is that of reacting correctly when the wrong orientation was held at the moment of release. From the first set of figures above, it will be seen that of 232 trials when the wrong orientation was held, 167 or 71% were correct. The second set of figures shows that of 421 trials when the wrong orientation was held, 309 or 73% were successfully carried out. Such a high percentage places the results above the possibility of explanation by chance. Again, the fact that this type of behavior dominated for several days at a time indicates that something more than chance was manifesting itself. At 8 secs. delay, e.g., Jack made 10 correct reactions in one day, starting with wrong orientations. The following two days had 7 each; and the following two, 4 each. The next day the delay was increased to 9 secs. and 8 correct reactions with wrong orientations were made. Eight, 6, 4, 8 and 6 were the records of such reactions for the following days. The next day after these, 10 secs. delay was given. Here 9 correct reactions were made starting with wrong orientations. These are fair samples of the prevalence of this type of reaction.

In order to emphasize further the fact that Jack did not need to keep *any* observable part of his body in a constant position in order to react correctly, I shall give one day's record in detail (Table X). Often only the raccoon's hind feet—or one of them—remained constant. The interval of delay in this illustration is 10 secs. N.c. means no part of the body kept constant. H.f.c. means hind-feet constant. Rt.h.f.c. means right hind-foot constant. If the letter designating the orientation held at the moment of release is in italics, the animal oscillated between two faces of the release. If in addition the letter is starred, the animal oscillated from one to another of all three faces of the release. On the 3rd and 5th trials, Jack was distracted by some noise during the delay. A concrete statement of what these symbols mean in one or more instances should make the record perfectly clear. On the first trial, the light was turned on in the right-hand box. It was turned off and the

animal held for 10 secs. During this delay, Jack turned toward all three boxes. No observable part of his body remained constant. At the instant of release, he was headed toward the proper box, a, and reacted correctly. On the 5th trial, Jack was oriented toward the middle box, *b*, at the moment of release. He went to *a*, then to *b*, then to *c* and back to the experimenter for food. The reaction had failed. If to the large number of correct trials made with wrong orientations be added the correct trials made with correct orientations that had been completely lost and then refound, the number of reactions significantly different from those of the rats and the dogs reaches relatively huge proportions.

TABLE X

Raccoon	Orient. when released	Light box	Behavior	
Jack	**a*	a	a	n.c.
	**a*	a	a	h.f.c.
	**a*	a	a	h.f.c.
	**a*	b	b	rt.h.f.c.
	**b*	c	abc	n.c.
	**b*	c	c	rt.h.f.c.
	c	b	b	n.c.
	**c*	a	a	n.c.
	b	c	c	h.f.c.
	**a*	c	c	n.c.
	b	c	c	h.f.c.
	a	b	b	rt.h.f.c.
	**b*	a	a	h.f.c.
	b	b	b	n.c.
	**a*	b	ab	rt.h.f.c.

Two other types of reaction remain to be indicated in the above table, (X). Orientation b was held 6 times; orientation a, 7 times, and c, twice. Yet different correct reactions followed from the same orientations. Jack was oriented to c once and went to b correctly. The next trial he had the same orientation and went to a correctly. In neither case had he seemed to keep any part of his body constant. In the 4 correct responses to the light box b, the corresponding orientations were as different as possible. Twice Jack was oriented to a and once each to b and c, yet each time the correct reaction was made. In other words, the *same orientation* did lead to *different reactions* and *different orientations* to the *same reaction*. These, again, are types of behavior never met with in the rats and dogs. This behavior occurred so frequently with Jack and with Bob—

less frequently, however, with Jill and Betty—that it must be described as a genuinely new type of reaction in these experiments and not as the result of chance or of the foreknowledge of what box was to be presented. (This latter possibility was adequately ruled out by controls.)

Table XI gives a numerical statement of the importance of orientation for the rats, dogs and raccoons. As far as this factor is concerned, the animals within each class were on a par with one another. For this reason, I give the results for typical subjects and not an average for all members of a group. The data given for each animal are calculated from comparable groups of 800 delayed reactions each.

TABLE XI

	Rat No. 14	Dog Blackie	Raccoon Jack
No. of reactions in accordance with orientation..	698 trials or 87%	760 trials or 95%	614 trials or 76%
No. of reactions not in accordance with orientation[43]	102 trials or 13%	40 trials or 5%	186 trials or 24%
No. of reactions not in accordance with orientation that succeeded	15 trials or 14%	5 trials or 12%	167 trials or 89%

This table indicates plainly the similarity of the behavior of the dogs and rats as well as the wide divergence of the raccoons from the other two groups of animals. The rats and dogs almost never reacted in opposition to orientation. When they did do so, the number of their successes was a negligible quantity. That orientation was a strong factor with the raccoons is evidenced by the 76% of reactions that followed it. This fact makes the 89% of correct reactions starting from wrong orientations of great significance. The reactions succeeded in opposition to strong orientation influence. This statement is supported by the facts above noted. It was shown there that one of the raccoons (a typical one): (1) Made different correct reactions from the same orientation, and (2) made the same correct response from different orientations. Additional evi-

[43] The large percentage of reactions not in accordance with orientation made by the rat, when compared with those made by the dog, is due to the acquisition of a habit of holding orientation b and reacting to box a, i.e., to a position habit.

dence will be presented later when the effect of the size of the release upon the methods of delay is considered.

One other line of evidence which points to the uniqueness of the raccoon's behavior should be noticed here, viz., the growth of the methods of delay during the course of the experimentation. Did the rats and dogs rely upon orientation from the beginning of the tests? Was the non-orientation cue natural with the raccoons or acquired under the stress of circumstances? An examination of the records reveals the fact that from the beginning of the learning tests the rats and dogs reacted in accordance with orientation. There was no development of a new method as the delays were increased in length. There was simply an improvement in the facility with which the animal maintained its position for a given length of time. When the orientation was lost, it was no easier to react correctly at the last of the experimentation than at its beginning. In the case of the raccoons also there was no initiation of a new mode of response. There were, on the average, as many reactions per day during the learning period that did not follow the orientation as there were per day during the delayed reactions. The improvement that took place as the experimentation proceeded was in the accuracy of responses not in accordance with orientation, when these responses were made after a long delay. "Non-orientation reactions," therefore, seem to be *natural* with raccoons and thus seem to differentiate their behavior from that of the rats and dogs.

(b) Position in the box.—The data considered under the above title are those indicating the effect that the large release box had upon the methods of delay used by the various animals. It will be remembered that the large release gave the animals the freedom of the interior of the box and thus permitted them to take up a certain position in the box as well as to maintain a certain bodily orientation.

Four rats, both dogs and all of the raccoons save Betty were tested with this large release. Only three animals had their methods of behavior essentially modified: Rats 17 and 16 and the raccoon Jack. In no case, however, did an animal that had depended upon orientation for its cue begin the use of a non-orientation factor. What the modifications were will come out in the following descriptive summary.

An analysis of the behavior of the four rats when tested with the large release, brings to light the following facts: Save in four cases, No. 17 went in the direction in which his body was pointed at the moment of release. *This was true regardless of his position in the box.* Nos. 13 and 15 always went in the direction of body orientation. Hence position was only effective as an aid to the retention of body orientation. During the last week of the tests with this large release, No. 17 manifested an exceedingly interesting type of behavior. He needed to keep *neither position nor orientation constant in order to react correctly.* I could find no observable body cue by which the problem was solved. The first day of this particular week, 3 of the 10 trials were of this type, but for several days no more instances were noted Then they were again in evidence until the end of the work. The *correct* reactions made in this manner never exceeded 3 per day. That I was much astonished at the sudden appearance of this type of behavior in the rat goes without saying. To think that at the end of eight months' steady work, the animal should suddenly adopt a new mode of behavior! It was not many days, however, before the accumulated data made clear the explanation. *The rat always turned to its right and entered the first box that was encountered.* Thus the animal could be at the middle box, but with its nose slightly to the right of the door to this box, and when released it would whirl and go to the box on the right. In the same manner, the rat might have its nose just to the right of the door to the left box when the release was raised and turn and enter the middle box. The behavior of rat No. 16 differed from this only in the direction of turning. This rat always turned to the left and entered the first box that it came to. From these data, it is obvious that the animals were using motor cues to guide their reactions. Their behavior was practically, if not entirely, automatic.

There is no need to detail the results obtained with the dogs when the large release was used. Both animals always went up close to the release in front of the lighted box and then waited until released. Each followed the orientation of his head regardless of which box was nearest.

Among the raccoons, Jack's change of behavior was a shift from the use of a non-orientation cue to a position cue. The

change occurred just at the close of the tests with the large release. His usual behavior as well as his new form may be set forth as follows: Before the new release was used, Jack was being tested with boxes *a* (the right one) and *c* (the left one) only. When the large release was put on, the tests were still confined to these two boxes. This was necessary in order that the results be strictly comparable. Of the 510 trials given under these conditions, 56 were made: (1) In which no observable part of the animal's body remained in a constant position; and (2) in which the animal's position in the box was wrong at the moment of release. In every one of these 56 trials, i.e., the animal was both in front of the wrong box and headed away from the right one when the instant of release arrived. Twenty-six of the 56 were reacted to correctly. Chance will account for this number of reactions as far as the mathematics of the problem are concerned. However, from the point of view of the observer, the majority of the reactions looked like anything but chance behavior. This was especially true when Jack reacted correctly and yet was pointed directly away from the proper box. There was a directness and sureness about his reactions that hardly savors of chance. I do not urge these instances on the reader as evidence of non-accidental reactions. I simply note the fact of their presence and the impression that they made upon me. It is well to remember in addition, though, that since orientation was such a strong factor in determining Jack's reactions (see above, Table XI) that any responses in opposition to this must be given great weight.

Owing to the fact that only two boxes were used in this series and that they were located far apart, the raccoon had every incentive to rely solely upon his position in the apparatus for the reaction cue. This he soon did to a large extent. In every case where his position was constant (and correct) and his orientation changed, he reacted successfully. In other words, position was the determining factor. When this became evident, the experiment was stopped. *The raccoon had shifted the basis of his response so that I could detect its nature by observation.* Jack's behavior at this point was thus on a par with the dogs and rats. Had the tests been continued, all that could have been expected was the perfection of a habit of staying near the proper box. This did not appeal to me as a profitable goal of endeavor.

For further confirmation of the statements made above concerning the general methods of delay, I shall present the raccoon Bob's record with the large release in some detail. Three hundred and sixty trials were given on two boxes with this release. Of the 27 reactions made with the *wrong position and correct orientation*, 19 were correct. Of the 91 reactions made with the *right position and wrong orientation*, 81 were correct. Sixty-three times the reactions were preceded by positions and orientations that were both wrong. There are two possibilities here: (1) Orientation and position may favor the same box; (2) they may favor different boxes. Forty of the 63 were reactions of class 1, i.e., were initiated by orientations and positions that favored the same wrong box. Fifteen of the 40 were successful. In other words, although both position and orientation favored the same wrong box, Bob was able to overcome the handicap and make 37% correct reactions. The remaining 23 reactions were of class 2. Of the 23, 16 were successful. Where position and orientation were both wrong, but did not combine to favor the same box, Bob made 69% correct reactions. Of the 7 reactions of class 2 that failed, 5 were in accordance with position and 2 in accordance with orientation, i.e., 5 times the animal went to the box favored by position and the remaining times to the one favored by orientation.

In order that the reader may have a perfectly concrete presentation of the reactions with the large release, I will quote a day's record from the diary. The reactions were made at 8 secs. delay and with the exit doors from the light boxes, *b* and *c*, open. In the records obtained with the large release, some new symbols were used in describing the data placed in the

TABLE XIV

EIGHT SECONDS DELAY WITH LARGE RELEASE

Animal	Orient.	Light box	Behavior	
Bob	b a–2	b	b	h.f.c.
	b	c	c	n.c.
	b	b	b	h.f.c. (walked to *a* and back)
	b c–2	b	b	n.c.
	c	c	c	
	b	c	bc	n.c.
	b	b	b	
	c b–$\frac{3}{4}$	c	c	n.c.
	b	b	b	h.f.c.
	b	c	c	n.c.

"orientation" column. The first reaction, e.g., is to be read as follows: The light was turned on in *b*, the middle box. Bob was held for 8 secs. during which time he pivoted on his hind feet, swinging his body along in front of *b*. At the moment of release he was still in front of this box, but was oriented with his nose in the corner halfway to *a*, the box on the right. During the delay at the fourth reaction, no part of the raccoon's body was constant. He walked over in front of *a* and when released was in front of *b*, but his body was pointed to the corner half way to *c* at the moment of release. At the instant of release for the eighth reaction, Bob's body was pointed three-fourths of the way toward *c*. This brought his nose within a few inches of the entrance to that box, yet the reaction succeeded. In the tenth trial, as in the second, position and orientation both favored the same wrong box. In each case the reaction was correct.

The net result of the data presented here for Bob is an almost entirely conclusive proof of the statement that he does not wholly depend either upon bodily orientation or upon position in the box for the cues determinant of the subsequent reaction. The 37% made with the reactions of class 1 is very significant when one does not lose sight of the fact that both orientation and position were here *combined against* any other factor leading to a correct response. In class 2 where such a combination was not present, the percentage is conclusive as to the presence of some non-observable cue. This is but further data confirmative of that already presented above for Jack with the small and large release.

In view of the generally negative results obtained with the large release, as far as developing new and higher types of behavior is concerned, some one may say: (1) That if all of the animals had been started with the large release instead of with the small one, they would not have been so likely to develop gross motor cues to guide their reactions, and (2) that it is not surprising that no new type of behavior appeared after the animals were firmly in the grip of habits developed in the small release. To these criticisms I can only reply that any experiment must exhaust first one method and then another. Time did not permit the use of both methods here.

2. Tests with Children

A. Method of Experimentation.—Details of the method of experimentation here used may be presented as follows: When the subject was first brought into the room, the following instructions were given: We have a little game in here which we are going to play. You will stand in here, (I indicate the release box). When I release you by raising the gate, (I illustrate raising the gate), you are to go and push one of the buttons over on the wall. One of these buttons will make a noise. If, after I raise the gate, you *push the noisy button first*, I will give you some candy when you come back to me. But if you go first to some button that isn't noisy, then you must try again before you get the candy. So, you see, the game is to *push the noisy button first* and so get candy. Do you see now how we are to play the game? Run over there and push on some of the buttons. See, sometimes they make a noise and sometimes they don't. (I switch the buzzer on and off for all the buttons. The lights are not on yet, nor has the subject's attention been called to them.) Now let's try the game and see if you can *push the noisy button first.* (The child is placed in the release box. The light is switched on over the noisy button. The child is held 5 secs. in the release box before being set free.) These instructions were memorized by the experimenter; and, although parts were repeated several times to the subject, nothing that is not given above was told to the child.

It was found that with all save F, 6 preliminary trials were sufficient to familiarize the children with the apparatus and to overcome their timidity. (H's exception will be noted below.) F was given 35 trials extending over three days on the preliminary work of learning that the noisy button meant candy. By the end of that time the association was firmly established.

No fixed number of trials per day was given. The amount of the day's work was adjusted to the child's disposition and to the length of delays. No set number of trials was given at any stage of delays. In other words much the same method was used as for the first year's work with the raccoons, Bob and Betty. Delays were increased continuously until an error was made. At this point, they were either decreased at once or continued at their existent value for several trials before further change was made. The experimenter believes that so flexible

a method, when properly checked by careful observation of the subject so that the task is changed in constant sympathy with the subject's apparent needs, is excellently fitted to bring out what the subject *can* do naturally as opposed to what it can be *trained* to do. Of course the effects of training cannot, and need not, be eliminated. However, they are not so great in the method outlined above as they would be were many trials given at each stage. The difficulty here is the same as that mentioned when discussing the records for the raccoons viz., where only a few trials are given, the critic has a better chance to claim that the results are due to chance. Our conclusions will seek to avoid this criticism. But in the last analysis, the themselves must be their own justification.

B. Are the Results Obtained from Animals and Children Comparable?—In the light of the foregoing method and of careful observation of the children, the following points suggest themselves as the essential considerations in a relative estimate of the conditions under which the children and the other animals worked: (1) Fear.—This was overcome in the animals by the preliminary training; in the children, by kindliness and cheerfulness on the part of the experimenter and by the child's examination of the apparatus as described above. (2) Motive.—Hunger and punishment insured a maximum of effort on the part of the animals. Candy, words of praise from the experimenter and a desire to excel its companions incited the child to do its best. (3) Knowledge of the reaction desired.—The rats, dogs and raccoons had to learn everything by themselves. (a) The preliminary series acquainted them with the fact that there were three exits to the problem box and possibly also with the fact that only one of these would be open at a time. (b) In the regular learning tests, these animals had to associate the light and the open box in such a manner that the light became the sign of the open box. (c) In the delayed reaction tests, again, they had to learn that the open exit was always in the box which had been most recently lighted. If we turn now to the children, we find the following situation: (a) They were told of the push buttons which for their problem corresponded to the exits of the other experiment boxes. Where the animals had had to learn the fact of only one open exit by trial and error, the children were at least aided by being told

that only one button would make a noise. (b) In the regular learning series, the children had to acquire the association between noisy button and light on their own initiation. It must be remembered that the experimenter *never* mentioned the word light and never directed the subjects' attention to the lights as long as the experiments continued. (c) In the delayed reaction, also, the children were thrown on their own resources in the working out of the problem. (4) Treatment during the delay.—During the interval of delay the rats, dogs and raccoons were usually left strictly alone. Only in a few control tests was any effort made to distract them. However, uncontrollable noises occasionally intervened and disturbed the dogs and the raccoons. During the period of delay, the children were entertained by the experimenter by means of stories, the drawing of pictures and, in a few of the long delays, by gifts of candy. In his opinion, these distractions made the experiment more difficult, although it is true that impatience and fretting on the part of the subject were largely eliminated. It would thus seem that the only objection to this method is that possibly the distractions served to urge the child to form a "purpose to remember" sooner than would otherwise have been the case. I cannot deny this as a possibility. It may have occurred with the girl M. But from the fact that the others remained impatient and complained of the delay until late in the experimentation, I do not believe they realized before that time that the problem was to see how long they could remember. Questioning on this point at the close of the experimentation confirmed this. (5) Possibility of the problem being talked over by the subjects.—This, of course, has no bearing on the case of the animals, but it presents a fairly serious possibility with respect to the children. The possibility is all the greater because of the fact that four of the children lived in the same neighborhood. Although recognizing this, I feel that it played very little part in the experiments; for where it might have been effective, the tests continued but ten days. M was old enough to do as she was told, unless severe temptation came her way. Such a possibility, however, was prevented by cold weather and school keeping her away from the other children. As will be pointed out later, talking could not have influenced H's conduct because she knew all about the experiment. F was under

the experimenter's control and never saw the other children. Hd and L differed so in their attitudes toward the problem and in their general behavior, that I cannot well believe they talked about the problem when together. Further evidence why it is improbable that the children planned with one another how to work the problem is that there was a very keen rivalry as to who should have the most candy beans at the end of the day's work. When the work was finally dropped, the children said that they had not talked the problem over with one another. In the light of these considerations, I believe that the children and the animals worked essentially on a par, so far as extra-individual influences are concerned, throughout both the regular learning series and the delayed reaction series. I say "essentially on a par" because the social influence due to the presence of another member of the same species was operative in the case of the children while it did not appear with the animals.

C. Learning.—A few words will suffice to describe the trials given on learning the association between the lights and the noisy button. With all subjects save F the light was first turned on at *a*. (For F it was put on at *b*.) On this first trial all the subjects failed. F went to *a*, then walked past *b* to *c* and thence back to *b* and rang the buzzer. All the others went first to *b* and then to *a*. This may have been due to the fact that *b* was the nearest of the buttons. F was the only one that made an error in this series after the first trial. The others learned the association in one trial. Out of the 16 trials on the first day, F failed on 9. The first test was missed the second day and none the third day. In other words, no errors were made after the 17th trial.

D. Differences Between the Learning of Animals and Children.—Some of the difference between the above data and that for the animals is undoubtedly due to differences in attitude toward the problem, although the conditions were so arranged that this should have been at a minimum. Five rats learned the association in from 160 to 176 trials. The two dogs, it will be recalled, required more than 500 trials. Bob received only 120 trials. These figures present the number of tests after which no errors were made. Over against these figures, 46 should,

stand as the largest number of trials given on learning to any of the children, and this to F, the youngest.

I believe that the main factor that would make for non-comparability in these results is that of brightness preference. It is not known what the value of this factor was for the children. But leaving this possibility aside, it is to be noted that the above records fall into two well defined groups; those for the animals and those for the children. If the difference here is correlated with grades of intelligence,[44] one may well ask why no such differences appear between the several classes of animals. The problem is all the more interesting when it is pointed out that there seems no certain correlation between the ability to learn the association and the ability to delay when one considers the various groups of animals. The dogs and the rats used the same method in delay, yet Blackie delayed longer than any rat. Jack and Bob used methods of behavior in delay quite different from the other animals and their delays were far longer than those of the rats and of Brownie. Furthermore two of these raccoons delayed about the same period of time, yet varied greatly in their times for learning the association. Among the children, matters would seem to be different. They learned rapidly and in delays reached relatively long periods of time by what seemed the same method used by the raccoons. The following answers suggest themselves with reference to the question put just above: The present data indicate: (1) Either that the different grades of intelligence among the animals were not great enough to be registered in the learning rates although the grades between animals and children were sufficient to manifest themselves; or (2) that the association can be learned with a type of process that will not suffice for long delays where the orientation is not maintained. If this latter alternative be correct, it would seem that the children used the method required for the last mentioned type of delay in their learning period. The raccoons, on the other hand, used the same method

[44] V. C. Hicks and H. A. Carr [45] find no such correlation of the number of trials taken in learning a *maze* and the grade of intelligence of the subjects. The time is not ripe for a statement of the type of learning present in maze problems when compared with the present test. However, the data presented by the above writers and by myself would indicate that the two problems do not involve the same means of solution, at least to the same extent.

[45] Hicks, V. C. and Carr, H. A. Human Reactions in a Maze. *Jour. of Animal Behavior*, 1912, vol. 2, p. 101.

in learning the association that the other animals did, but a different method when it came to delays. There may thus be a difference in intelligence due to varying abilities in the use of some one instrument of adjustment or due to varying abilities in the use of different instruments of adjustment. Blackie and the rats would illustrate the first case; the children and raccoons in comparison with the other animals would illustrate the second. So much for the possible meaning of the learning times of the different animals and children

E. Delayed Reactions.—M, age 8 years, was given 38 trials on delay. The first 2 only were at intervals less than 1 sec. The others ranged from 1 sec. to 28 mins. and extended over 7 days. Fifteen trials were on intervals of 10 mins. and over. Only two errors were made, one at 5 mins. and one at 20 mins. The latter was probably caused by ill-humor. Longer delays might doubtlessly have been secured had it been desired. The only attempt was as follows: On four days (not consecutive ones), M was asked which button she pushed last the day before. Three times she answered correctly.

M did not need to keep any part of her body constant during the delay. On the trials involving more than 10 mins. delay, she was sent out of the room and put with the other children. During the delays she conversed freely with the experimenter. Any "purpose to remember" that the subject formed was formed on her own initiative. Great care was taken—and I believe effectively so—to insure that no suggestions be secured from extraneous sources. As early as the 15th trial on delays M volunteered the information that she remembered where the light was in order to push the proper button. Several times she reacted correctly and then volunteered that she had *guessed* which button to push.

Several extra trials were given in which the problem was changed. The light was now placed successively over two different buttons, the last button being the noisy one. This problem brought out a type of reaction very often seen in the animals. M would start toward one box and then turn and go to another. In the regular series, hesitation occasionally occurred and M wavered between two boxes.

Hd, aged 6 years, was given 47 trials on delays. Only the first two were less than 1 sec. long. The others ranged from

1 sec. to 33 mins. Fourteen trials involved intervals of 10 mins. or over. A total of 15 trials was given with intervals of 4, 5 and 6 mins. in length. In the 47 trials, 10 errors were made. Two of these came in each of the delays of 4, 5 and 6 mins. One came at 11 mins.; 1 at 12 mins.; and 2 at 20 mins. Hd, therefore, had no trouble in remembering the solution of the problem until the intervals of delay reached 4 to 6 mins. Then the difficulties that arose were mastered and did not reappear until the periods 11 to 12 mins. and 20 mins. were reached.

Hd did not need to keep any part of his body constant during delay. He and the experimenter exchanged stories continually. This subject differed from M in the number of errors and in the greater frequency with which information was offered. The former I attribute to difference in ability; the latter, to natural garrulousness. The restraint during delay was a great source of annoyance to Hd. He complained a great deal because of it. My diary notes contain many such passages as the following: When light was turned off, he said: "Why hold me so long, I may forget which it is." The subject evidently realized that he was to remember where the light had been. Many times during the delay, Hd would stop conversing and say "O, I know which one it is" and would then point—not always correctly—to the button he had in mind. Instances of wavering and hesitations preceding acts of choice were noticed with this subject.

L, age 6 years, was given 41 trials on delay. Only one of these involved an interval less than 1 sec. long. The remainder were from 1 sec. to 25 mins. in length. Twenty-one trials were on delays from 4 to 9 mins. long. Nine were on delays over 10 mins. in length. Seven errors were made in the total 41 trials. All of these came in delays of 4 mins. or over. Two were at 4 mins. and one each at the following intervals: 5, 7, 8, 15, 17 and 25 mins. L, as Hd, found most difficulty in the period around 5 mins.

L did not find it necessary to keep either his body or the direction of his attention constant in order to solve the problem. He, too, conversed freely with the experimenter during delays. L would often watch for the light out of the corner of his eyes. After it appeared, he would apparently pay no further attention to the problem until released. L was less demonstrat ve

than Hd and did not remark so often during the delays that he still knew the proper button. Like Hd, he was impatient during the short delays at the first and the long delays at the last of the experiments. At times he wavered and hesitated in his reactions, saying that he had forgotten or that he wasn't sure. (In such cases, of course, the experimenter gave no cue to the solution.)

Each of the three subjects whose records have been discussed formulated his own "purpose to remember." H was given trials under the same conditions as the others, save that she was told the purpose of the experiment. She was told that the light would be over the noisy button. When delays were begun, she was told to be sure and remember where the light had been.

H, age 6 years, received 15 trials on delays. Only the first two involved intervals less than 1 sec. in length. The others varied from 10 secs. to 35 mins. One error was made at 21 mins. At this trial, H walked half way to *b*, paused for 5 secs., wavered as though to go to *c*, but finally pushed *b*. She then returned to the release and was told to try again. Again she wavered at a point half way to *b*, but this time she went to *c*. The light had only been turned on the one time. I did not talk to H during the first 12 delays. She was always told before the light went out to be sure and remember, but this was all. In the subsequent trials, every effort was made to distract her during the periods of delay. The results obtained with H, when compared with those for Hd and L, indicate that it is an aid to the subject to have the "purpose to remember" expressly formulated for him.

F, age 2½ years, was given 507 trials on delay. Of these, 30 delays were less than 1 sec. long. The other 477 trials ranged from 1 sec. to 1 min. Of the 477, 143 were wrong. The following table (XV), gives the relative distribution of these errors. It does not give the delays in the order in which they were given to F. The table simply summarizes the number of trials and errors at each stage. An advance was never made from one stage to another until at least 80% correct reactions were made for at least 5 successive trials. The only interval that F did not finally master was 1 min. This is surprising when such a high percentage of correct reactions occurred at 50 secs. On each of the two days when F was tested for the 1 min. delays,

TABLE XV

Delay	No. of trs.	Errors	Delay	No. of trs.	Errors
1 sec.	23	4	15 secs.	65	19
2 secs.	4	0	20 "	40	8
3 "	5	0	25 "	22	5
5 "	38	10	30 "	22	11
6 "	9	0	35 "	18	2
7 "	35	9	40 "	23	10
8 "	3	0	50 "	19	2
10 "	85	37	1 min.	45	26
12 "	10	0			

the first one-third of the trials were at 50 secs. Reactions were perfect. The last two thirds of the day's work was at 1 min. and both times she fell below 50%. There were no known extraneous factors to cause this. The conditions were as near ideal as possible. The method adopted with F, after the first three days, was one of slow advance from stage to stage. At the higher delays, each day's work was begun—as just illustrated—with the longest interval that had been mastered. Some 5 trials were given here and only if the reactions were perfect was an advance made. I believe that entire dependence can be placed upon the results obtained. F was the only one of the children that did not reach a delay of at least 20 mins.

F was distracted continually. She needed to keep neither her attention nor any part of her body constant in order to react correctly on delays up through 50 secs. She often hesitated and wavered in making the choice of buttons. It is interesting to note in this connection that this wavering, hesitant behavior was only noted in the case of the raccoons and children. Every individual of these gave many examples of it. This is significant when it is borne in mind that these subjects used apparently the same method in solving the problem. If that statement is too broad, at least it may be said that they agreed in *not* using gross motor orientation exclusively as did the rats and dogs. This is not the very usual type of behavior described by Yerkes for the dancing mouse. He says: "I have at times seen a mouse run from one entrance to the other twenty times before making its choice; now and then it would start to enter one and, when half way in, draw back as if it had been shocked. Possibly merely touching the wires with its forepaws was re-

sponsible for this simulation of a reaction to the shock." [46] The above reaction, noted by Yerkes, and other similar cases in the literature are reactions to present stimuli. Where the mouse ran to the entrance of the box, but did not enter, the explanation undoubtedly lies in the inability of the stimulus to set off the proper reaction. As Yerkes suggests, the stopping of the animals half way to the box was probably due to the contact with the wires. The same general type of behavior described above for the raccoons and children occurred during certain periods of time with my own rats and the dogs. In every case, however, it was due to a habit of turning around when released. This habit was executed in the same fashion no matter which was the proper box to choose. As opposed to the tests where either present objective stimuli or habit are involved, the reactions of the children and raccoons were, *as far as could be determined*, perfectly spontaneous, i.e., determined by intra-organic conditions that varied for the different boxes. These cases belong in the same class as that of Miss Washburn's cat described above (p. 20).

F's father, a trained psychologist, informed me that the child was just reaching the stage where her memory for objects and events had begun to take on definite form. When brought in from a ride in the park or a visit to a friend, she could very seldom remember the details of the event, indeed not more than half the time could she remember the gross fact of having been somewhere. This occurred even with what were to F very interesting experiences. Sometimes, it is true, the difficulty lay with the lack of control of language; but this was not always the case. After her playmate had gone for some time, if F was asked who had been there, not only could she frequently not tell, but at times she was bewildered even by the suggestion that anyone had been to see her at all. On the other hand, some cases were noted where F remembered an event for several days. Phrases, also, that she had heard but once were often spontaneously repeated for the first time several days later. The present tests thus found the child in a very important stage of mental development. Definite memories, of the adult human kind, were still hanging in the balance with the chaos of the preceding period. As far as the lengths

[46] Yerkes, R. M. The Dancing Mouse. 1907, New York, p. 130.

of delay go, F ranks approximately half way between the other children and the raccoons. As to apparent method of work she—and the other children too—is superior to the raccoons in that orientation played no discernible rôle in her reactions.

VI. THEORETICAL CONSIDERATIONS

1. The Cues Essential for Successful Delayed Reactions

After the above presentation of experimental results, there remains the important task of determining as nearly as possible just what cues the subjects used in their reactions. The following paragraphs of this section will set forth in detail the various possibilities of interpretation and indicate what seem to me to be their relative validities.

Let us first formulate in a general way the conditions that a reagent will have to meet in the solution of the present problem. He is confronted with three boxes which offer as many known possible points of egress. One of the boxes is lighted. In the course of a series of experiments, the reagent has learned to go through, or to the lighted box in order to reach food. In time the stimulus will set off the reaction practically automatically. Approximately at this period, I begin to turn off the light before the subject reaches the box. It is possible that for several stages of delay the box which has been lighted remains light for a short interval of time due to the presence of after-images in the subject's eyes. But as the delays increase in length, there will come a point at which the problem shifts from "go to the lighted box" to "go to the one of three dark boxes that was most recently lighted." If this shift comes before the 1 sec. delay, it will be less likely to involve relearning. If, e.g., the change comes at the second stage, the momentum of running will make it easier for the animal to continue into the dark box than to turn and seek another. With some rats, the results indicate that the change did come at this stage; for when the light was turned out, the animals stopped short and went into another box, even though that too was dark. With the other animals and the children, it was impossible to tell just when the shift came. The problem having once changed, however, the question now is how long after the boxes all become dark can the subject pick out the one which was lighted most recently. Our special problem concerns only the solution of this

second problem. During the learning series, the light stimulus acquires the power of releasing muscular activity applied in a certain direction. Just what direction this shall be is determined by the spatial location of the light. A significant meaning here attaches to the object ve stimulus, i.e., the light plus the definite location. (By the term meaning, I imply nothing more than the fact that a certain stimulus evokes a certain reaction under conditions that are not usually described as involving mere habitual or reflex activities.)

As soon as the problem shifts to a choice of one of three similar boxes, i.e., as soon as a stage is reached where the determining stimuli are absent at the moment of reaction, then it is necessary, I assume, if the reactions are to succeed, that the subject develop substitutes which shall take the place of those stimuli as carriers of the needed meanings. In other words, the substitutes must *fulfill the function* of the previous stimuli in arousing the three appropriate movements. The substitutes may secure this power either through association with the light during the learning series, or during the delayed reaction tests themselves through a process of trial and error. In view of the fact that delayed reactions did succeed under the present conditions, there can be no question as to the *existence* of the substitutes. Our next problem is that of determining their nature.

The substitutes or cues that determined the subject's reactions may theoretically have arisen either within or without the organism. We shall consider the latter first.

A. Substitutes Derived from the External Environment.—Under this heading, we shall consider two possibilities: (a) Were there three simultaneously present objective cues that may have served to *determine* the subject's reactions? and (b) were there three objective cues that varied from trial to trial with the position of the light which may have determined the reactions after the light was turned off? In any case there must be two or three cues each determining one of three reactions. We shall consider the possibilities in the above order.

(a) Simultaneously present objective cues.—Although every effort was made to secure uniformity in the visual appearance of the three light boxes, they differed at least in spatial position and in the nature of the part of the experimental room visible above the walls of the apparatus. In addition there were pos-

sibly olfactory differences in the boxes used by the animals due to the animal odor itself and (in the case of the rats and raccoons) to a food odor set up by the milk that was on the forefeet of these animals. Suppose now that these simultaneously present external factors be treated in their entirety for each box and be represented by the letters x, y and z. It *may* be assumed that the subjects learned to react not to the lights—a, b and c—alone, but to the complexes ax, by and cz. Then when the delays were begun, the reactions were made to x, y and z, either immediately or after a period of learning. Here we would have three cues governing as many reactions, and all three cues would be presented simultaneously at the moment of release whereupon the reaction would take place.

This hypothesis is not supported by the experimental facts. If the reactions during the delay were determined by these stimuli present at the moment of release, the animals should have learned to delay for almost any interval of time. It should be no more difficult to react to x, y and z, than to the lights, since the reactions during delays were just as "precipitate" and "headlong" as when the lights were present. Now it was demonstrated that all of the animals could react perfectly when held in the release 1 min. with the light on. Why then could they not delay a minute with the factors x, y and z which were also constantly present? Further, if the subjects succeeded in reacting to x, y and z for intervals of 5 or 6 secs., why should they be unable to reach 7 or 8 secs.? In other words, why, if they reached one stage of delay on this basis, should the subjects not go a little beyond and so up to a large delay? There is no answer to this, if one assumes, as we have done, that the cues are simultaneously present at the moment of response.

In the section on experimental results, tests were described where the entire sides of the problem box surrounding the entrances to the light boxes were covered with cardboards of widely separated grades of brightness. This device accentuated (from the experimenter's point of view) the constant differences between the hypothetical stimuli x, y and z, yet under these conditions neither rats, dogs nor raccoons showed improvement in their abilities to react. This series does not prove that no objective factors x, y and z were influential in initiating behavior; but, in conjunction with the immediately following theoretical discussion, it does make it very improbable that such fac-

tors should have exercised a determining influence on the reactions.

Let us grant for the moment that such objective factors were present, could they *alone* control successful reactions, i.e., could they be the *determining* factors in initiating correct delayed reactions? Since x, y and z are present *simultaneously* at every response, they *per se* cannot be the bases for differential responses. The subject must *select* one of the boxes. Hence the hypothesis would need to assume that the subjects are not reacting merely to x, but to x-where-the-light-has-just-been. Here we are forced face to face with the problem from which we started: What represents, or is a substitute for, the light? What is the element attaching *to* x that is equivalent to "where-the-light-has just-been"? There can be no question but that x, y and z in some form constituted a part of the general stimulus, for the subject must apprehend the different spatial locations of the boxes in order to react to them. The objection may be offered that this contention as to the effectiveness of these objective stimuli is inconclusive, inasmuch as the animals may have been reacting entirely in terms of kinaesthesis, i.e., had learned the fixed order in which the three boxes were presented. This possibility was adequately eliminated by control tests whose results are discussed on p. 67. The point to be established here is that x, y and z could not have been the crucial substitutes for the lights. These must have been factors which were not all simultaneously present, each in its entirety, at every response, i.e., they must have alternated from trial to trial, depending therefore on the position of the light. The question now is: Were there any such factors in the objective environment?

(b) Alternating objective cues.—Controls were instituted: (1) To prove that during the learning series the light (an alternating factor) was the determining cue for the reactions; and (2), to prove that in the absence of the light no other external factor took its place as a determinant of the reactions.

1. The reagents did not derive any cues from the experimenter. Screens were so arranged that the operator was never visible to the dogs. Control tests were also made under these conditions with the rats, raccoons and children. The constant beating of a metronome covered up any noises due to the ex-

perimenter's breathing that the subjects might have utilized. The dogs and raccoons did not rely upon the *manner* of being released. This was proved by having different persons operate the release. This control was not used with the rats. However, it is extremely improbable that these animals depended upon cues from such a source. Moreover had they or any of the reagents done so, their intervals of delay should have been almost indefinitely great. It must be borne in mind that any cues derived from the experimenter, in order to afford extra aid in the reactions, must be present at the moment of release. This would make it possible for the animal to avail itself of the cue after a delay of any length. If the cues were given by the experimenter only at the beginning of the delay, the problem confronting the animal would not differ from that of delaying with reference to the light.

2. The reagents did not depend upon any after-glow of the lights. When the lights were left on for one minute—a period much greater than was ever used in the experiments proper—and were then turned off, there was no appreciable after-glow of the carbon filament that the experimenter could detect. (In any event, such an afterglow would not persist long enough to influence the longer delays of the reagents.) Hence any continued brightness of the boxes (considered from the reagent's point of view) after the current was switched off must have been due to the reagent's after-images. The possibility of using these for cues will be considered below.

3. The reagents did not depend upon variations in the temperature of the boxes in making their reactions. After the lights had been turned on for one minute in any box, the temperature of that box was never raised more than a degree centigrade. Indeed only occasionally could any change of temperature be detected. The "headlong" manner in which the animals reacted to the boxes, together with the fact that they oriented toward the light immediately upon its appearance indicate that they were not governed in their reactions by the slight variations in temperature. Yoakum[47] found that rats could discriminate differences of 16° C., but even then their behavior was the result of long special training. His squirrels

[47] Yoakum, C. S. Some Experiments Upon the Behavior of Squirrels. *Jour. Comp. Neur. and Psych.*, 1909, vol. 19, p. 565.

—and probably the rats also, although he does not say—were hesitant about entering the boxes even with a difference of 25° C. These data make it exceedingly improbable that the animals in the present tests were influenced by temperature.

4. The animals did not derive cues from the doors at the exits of the light boxes. Numerous control tests were made in which all of the doors—and they were of large wire mesh, and hence hard to see—were open. Under these conditions, the animals reacted as though only one door had been open, as was usually the case.

B. Substitutes Derived from within the Subject's Body.—The discussion of these intraorganic substitutes will be divided into two parts: (1) What were the internal cues used by the various reagents?; and (2), how did these internal cues operate in order to guide behavior?

(a) The type of internal cue used.—1. Did the subject anticipate the order of presentation of the lights, i.e., were the cues to the reactions the individual responses of an habitual series? The evidence is perfectly unambiguous in support of the fact that the reagents did not rely upon the presentation order of the boxes in making their reactions. The order for each group of animals (not children) followed a series of 30 presentations so arranged that each box recurred an equal number of times. A new series was given whenever one box was eliminated from the experiments. Control tests in which the regular order of presentation was varied were introduced practically once every two weeks with all the animals. Controls were also made in which the lights were not used.

2. Did the reagents guide their reactions by after-images of the light? Although the existence of after-images in animals has not been demonstrated, we shall, for the sake of the argument, disregard this fact and admit of their possibility. In order to lead to correct reactions in our experiment, these after-images must appear in the proper direction for each response. Their directional position is a function of the orientation of the head and eyes, and as a consequence the hypothesis can not explain those correct reactions resulting from faulty orientations. After-images can possess a possible function only in conjunction with the maintenance of a constant orientation, and the hypothesis would need to assume that these overt motor attitudes are but

subsidiary phenomena serving as a means for the effective functioning of the after-image processes. But after-images can hardly persist long enough to account for the maximum periods of delay attained by this method of solution. Such delays varied from 10 secs. for the rats to 25 secs. for the raccoons and 5 mins. for one dog. All of the conditions of the test were distinctly unfavorable for any persistence of possible after-images. The light was weak in intensity (3 c.p.). Its average duration of exposure was approximately but 5 secs. Any approximation to steady fixation either during or subsequent to the exposure of the light was the exception. The animals as a rule were continually on the move, nosing and clawing at the face of the release box both during the exposure and the period of delay. Steadiness of fixation after the perception of the stimulus is a very essential condition for the development of after-images. Movements of the head or eyes tends not only to prevent their appearance but also to destroy them when present. The significance of these conditions is more apparent by recalling the fact that any extended duration of after-images is an exceedingly rare phenomenon in the normal perceptual activities of humans. Fixation is too short and changeable for their development. Since we are forced to argue from analogy with human conditions, one must also distinguish between the possible presence of such processes and the ability to perceive them. The mind tends in the interest of clear vision to overlook and neglect such processes as it does in the case of entoptic phenomena. With many people the ability to see after-images involves a previous knowledge of their existence and some degree of training and practice in their observation. In other words, after-images as persistent objects of consciousness are a product of the laboratory, and the assumption of their effective existence as guides to conduct in the normal perceptual activity of an animal is exceedingly questionable. After-images exhibit the phenomenon of intermittence. No high percentage of correctness of response could result from such a cue, as its presence at the moment of response would be a chance coincidence. The theory of after-images, moreover, is entirely unnecessary, as we have the possibility that these motor attitudes of orientation may themselves serve as a sufficient guide to

conduct, and on this hypothesis the assumption of the effective presence of after-images is an explanatory luxury.

3. Motor attitudes of orientation as cues of response. The data of the preceding section conclusively prove that maintenance of orientation during delay was an essential condition for correct response with the rats and dogs, and that such motor attitudes exerted a strong influence upon the behavior of the raccoons. Either these attitudes serve as the substituted cues and control conduct directly, or they function indirectly as a means of support to some such cue as an after-image. The evidence unambiguously favors the first supposition. These orientation attitudes, like any sensory process, may be a stimulus to definite movements. This tendency of the animal to run in the direction of their orientation at the moment of release was natural and habitual. The tendency was present in full strength at the beginning of the experiment. The tests merely developed the maintenance of orientation for longer and longer periods. This fact indicates that the motor attitude functions directly upon subsequent conduct. If the attitudes were but a means of support to some other cue, one would expect that this relationship of means and end would need to be acquired gradually during the experiment. An alternative theory presents many theoretical and factual objections. Any such roundabout and forced type of explanation is entirely unnecessary when we know that motor attitudes are a natural guide to the direction of subsequent responses. The mechanism of such a cue may be entirely automatic and mechanical. It requires nothing more for its explanation than does any habit. A stimulus initiates a certain act whose completion is prevented by external means. This initial activity persists unchanged so far as its directional aspect is concerned until the raising of the release permits it to function in a normal and habitual manner.

4. Some unknown intra-organic cue non-observable by the experimenter. Our data prove conclusively that some such cue was utilized by the raccoons and the children. Our proof of this statement is based upon the method of exclusion and the nature of such a factor must necessarily be defined at present in negative terms. We have exhausted our ingenuity as to objective possibilities of explanation, and as a consequence

are forced to conclude in favor of an intra-organic factor. The possibility of a temporal series of habits was eliminated by control tests. Neither orientation nor any distinctive motor attitude could be detected in the children or in at least 25% of the responses of the raccoons. The after-image hypothesis is entirely inadequate when orientation is faulty. In those reactions of the raccoons resulting from wrong orientations, the percentage of correctness was so great as to eliminate the possibility of chance. The nature and mechanism of this factor will be discussed in subsequent sections.

5. The following table (XIII) summarizes the cues used by the different reagents. P.C. (possible cue) means that the reagents so listed may have used that cue at times. N.C. (necessary cue) means that the reagents so listed had to use that cue or fail in a significant number of their reactions. R stands for rats; D, for dogs; RA, for raccoons; and CH, for children. For convenience of reference a classification is made

TABLE XIII

Cues Used by the Re-agents

External	Internal							
	After-image		Orient. At.		Non-orient. At.		Idea	
	P.C.	N.C.	P.C.	N.C.	P.C.	N.C.	P.C.	N.C.
Never a determining cue	R D RA CH		RA	R D	RA CH		RA CH	

in this table of those reagents that may have guided their reaction by ideas. This phase of the table will not be clear until the final section on the Place of Ideas in the Grades of Animal Learning is read.

(b) The mechanism by which internal cues guide behavior.—As already indicated the mechanism of orientation attitudes presents no difficulties. The light stimulus arouses the proper act. This inhibited act persists unchanged so far as its essentials are concerned during the delay. At the moment of release the animal runs in the direction of its orientation. This tendency of responding in conformity with orientation is natural and

habitual with the animal. The whole process is explicable on the basis of habit. Maintenance of orientation is acquired gradually by the trial and error method as is any habit.

The case of the non-orientation cues presents more difficulty. During the preliminary learning tests there was established by the trial and error method an association between the lights and the three acts of securing food. Between the two terms of each of these three primary associations there was interpolated probably by the trial and error method an intermediary link. These three cues were associated with their respective lights on the one hand and their respective acts on the other. Each light will now awaken its corresponding cue and this cue will in turn initiate the act with which it has been associated. In order to insure correctness of response, the proper cue must be present at or immediately after the release. As the interval of delay between the light stimulus and the response is increased in length, we have three possibilities as to the behavior of the intermediary link or cue. (1) After being aroused by the light stimulus, the cue may persist, or be constantly maintained, during the interval of delay. All of the available evidence tends to disprove such a hypothesis. The raccoons were frequently distracted during the delays by various laboratory noises, such as the squealing of rats and the rattling of windows. Note was made of these occurrences and still correct responses were possible in spite of these distractions. The raccoons were exceedingly active during the delays, pawing and clawing and running all about the release box. I often distracted the animals by bending down over the release box and yelling at them at the top of my voice. A typical case occurred when Bob was making a delay of 15 secs.—with a very high percentage of correct responses. These distractions during delay lowered his percentage approximately eight points. His behavior indicated that this treatment actually diverted his "attention" from the problem at hand. The emotional character of such a disturbance makes the high percentage of correct behavior especially significant. I also continually distracted the children during the delays by engaging their attention with stories, drawing pictures, conversation, etc. In fact the attention and interest of the children were often engaged to the point of absorption by these devices with no effect upon the correctness of their reactions.

The constant maintenance of the cue under these conditions of distraction and length of delay is highly improbable. Speaking in conscious terms, it would require great concentration and mental ability even for a human adult to keep any cue constantly in "mind" during such conditions. (2) The cue might be some intrinsically intermittent process such as an after-image. Such substitutes, however, could not suffice to guide reactions under the conditions of the experiment. Their presence at the moment of release would be purely accidental and hence they can not account for the high percentage of correct responses obtained. (3) We are forced to adopt the third hypothesis that the cue disappears after being aroused by the light stimulus, and is rearoused in some manner at the moment of release. To explain the mechanism of this revival, we shall assume that all three of the intra-organic cues have become associated during the course of the experiment with some sensory factor connected with the releasing of the animal. Hence the release is a stimulus which tends to arouse all three intra-organic tendencies. This revival, however, must be selective and adaptive and this adaptiveness can be explained by two additional assumptions. The presence of each light stimulus at the beginning of delay excites its corresponding intra-organic factor, and this excitement subsides after the disappearance of the light. Although the release stimulus does *tend* to revive all three factors, yet it will arouse that one most *recently active*, viz., that excited by the light at the beginning of the delay. The assumption that the predisposition of a tendency to response depends upon its recency of functional activity is a recognized principle of human psychology.[49] With such a mechanism, it would seem that the problem of delay should present no serious difficulties. However, the time interval between tests, i.e., the differences between the recency of excitation of the three factors, is small in many cases. Learning to enter the box most recently *lighted* as opposed to the box most recently *entered* is also no easy problem to master.

As we have indicated, such a mechanism would apply only to the non-orientation cues used by the raccoons and children. The type of function here involved is *ideational* in character.

[49] Ladd and Woodworth. Elements of Physiological Psychology. New York, 1911, p. 285.

By applying the term "ideas" to these cues, I mean that they are similar to the memory idea of human experience so far as *function* and *mechanism* are concerned. They are the residual effects of sensory stimuli which are retained and which may be subsequently reexcited. The revival, moreover, is selective and adaptive to the solution of a definite problem, and when aroused, they function successfully as a necessary substitute for a definite component of the objective stimulus aspect of the problem. The question as to the *content nature* of these cues, i.e., their sensory or imaginal character, is reserved for the succeeding section.

2. The Place of Ideas in the Grades of Animal Learning

A survey of animal reactions from those of the protozoa to those of the higher vertebrates leads one to the conclusion that the simplest behavior, from a genetic point of view, is the adjustment of a certain movement to a certain object or situation. This adjustment may be either native or acquired. In the former case, it is instinct; in the latter, the result of individual learning. (As such, it does not seem to be present in the protozoa.) The remarks in what is to follow are directed solely to the latter case. In the literature this is referred to as the stage of learning by experience on a sensori-motor level. Over against this genetically simple learning, may be placed a more complex form of behavior which involves a representative function. This ideational or representative process arises out of a genetically prior sensori-motor level of behavior. The field of its functioning is limited, moreover, to the representation either of some aspect of the object (or sensory) side or of some part of the movement and its consequences. In other words, the representative process must stand for either the sensory or the motor aspect presented in the genetically lower level of behavior. According to the law of parsimony, the only conclusive evidence in favor of the existence of such a representative element is the case where successful adaptations occur when that part of the sensori-motor process assumed to be represented is known to be absent at the moment of response. If the object or movement to be represented is present, why assume a representative or ideational process? The adjustment can be explained in terms of sensory stimulus and response.

This is the defect in all of the arguments and experiments which we have examined in the historical section above. Ideas *may* have been present, but since all of the behavior *can* be interpreted in terms of stimulus and response, the arguments are inconclusive.

From this point of view, there are from the standpoint of function two classes of ideas:—ideas of objects or those representing the stimulus aspect of the situation, and ideas of movement or those representing some aspect of the movement or its sensory consequences. Theoretically, the problem of ideas can be attacked from either point of view. Many discussions and experiments do approach the topic exclusively from the movement side, but practically such a procedure involves almost insuperable difficulties. Washburn,[50] e.g., makes the number of ideas of movement possessed by an animal more or less of a rough index of that animal's place in the scale of intelligence. But the presence of such ideas is as yet an assumption of very uncertain validity. Furthermore, an experimental technique that would isolate and control the movement factor would be extremely difficult, if not impossible to devise. When, e.g., an animal is brought along a path in a maze to a point where two possible reactions are presented, both responses are for the moment inhibited. But the two movements need not be represented, they may be actually there, although in an incipient form only, i.e., the conflict may be between the motor impulses themselves and the conflict may be resolved on this level without the influence of any factor representative of the effects of the movements. I doubt whether experimental tech nique can ever control this movement factor. Quite the reverse is true with respect to controlling the presence of the object, i.e., of the determining stimulus. This latter may be given or withheld at the investigator's pleasure. It is the merit of the experiments here set forth to have followed such a procedure. In the present case, there seems to be no room for doubt that the object reacted to was the light. Now if a representative function were involved in the behavior of the reagents, as seems to have been the case with the raccoons and children, it must, in part at least, have been representative of the lighted box, because all else—including the three possibilities of movement—

[50] Washburn, M. F. Op. cit., pp. 279–284.

was constant from trial to trial, whereas a selective response must needs have an alternating cue.

In the light of the evidence in the present monograph, let us grant the presence, in certain reagents, of a process representative of objects. The question now arises, Must this process be imaginal or may it be sensory? We may treat the latter possibility in two ways: (1) There may be a sensation arising from the reagents body—kinaesthetic, e.g.,—that stands for a certain reaction. Or (2), the substitute may simply consist of a differential meaning attached to the perception of the particular light box. In this case, when the reagent apprehended the box, he would simply recognize it as the one in which the light had been most recently. This would be perceptual recognition as the term is understood in human psychology.

With either of the above explanations, it must be remembered, the question that we are now raising is one concerning the content of the representative factor. There is no doubt in my mind that the *function* is an ideational one. Even should some critic claim that all of the present behavior is but perceptual recognition, the fundamental difference between the behavior of the class containing the rats and dogs and that of the class containing the raccoons and children will still challenge explanation. If the behavior of both the above classes of reagents is to be termed perceptual recognition, then two orders of this must be admitted—one on a level with habit, the other on a level with ideas.

One class of facts suffices to disprove the possibility of accounting for the behavior of the raccoons and children on the basis of perceptual recognition. These reagents did not stop and search for the proper box. They started their reactions immediately upon being released. The raccoons Jack and Bob, e.g., might be headed *away* from the proper box at the moment of release and yet whirl around in the proper direction and react immediately. Bob, in particular, might go half way to the wrong box and then turn suddenly and react successfully. When the reagent does not follow his orientation, one would expect him usually to look toward at least two boxes before reacting, if the behavior were on the basis of perceptual recognition. It is these reactions of children and raccoons that were not in accordance with orientation and yet that were "head-

long" and "precipitate" that lead me to regard perceptual recognition of the boxes as an inadequate explanation of the facts.

The objections just stated do not apply to the assumption that the representative process is an intra-organic sensation. A positive justification of an ideational function whose content is an internal sensation may now be elaborated. There is no doubt but that in human consciousness a sensation may carry a meaning that is woven into thought sequences. In reading, e.g., all of the actually discernible conscious content may be kinaesthetic sensations from the muscles of the throat or may be auditory sensations, if the reading be aloud. Whether one say that the sensation is the meaning or that sensation is there plus a meaning, the case for our purpose is unaltered. Each sensation and its meaning become incorporated in a train of thought. A slightly different situation is presented where it becomes necessary for the sensation to represent that which is not there and then stimulating the sense organs. The following illustration from Titchener is a case in point: "I had to carry across the room, from bookshelf to typewriter, four references—three volume numbers of a magazine, three dates, and four page numbers. The volumes and years I said aloud, and then consigned to the care of the preservative tendencies. Of the four page numbers, I held two by visual images, one by auditory, and one by kinaesthesis."[51] The case of the volumes and years and that of the page numbers remembered by means of kinaesthesis are significant. Each is an instance (so far as we can tell from the account) of memory without the revival of images. The sensory cues, present at the time when the data were written on the machine, elicited the proper material. It is a dangerous procedure to complete another investigator's introspections. I do not intend to do so here. I simply suggest the above as a possible supplement to Titchener's own brief statements. Whether it be right or wrong in his case, the experience in which a *single* sensory process represents an absent object is sufficiently frequent to give us the suggestion for which we are seeking. The suggestion may also be found elsewhere. That which has become known as "conscious attitude" in the

[51] Titchener, E. B. Experimental Psychology of the Thought Processes. New York, 1909, p. 202.

literature of imageless thought is identical with the first phenomenon that we have had under discussion in this paragraph. So far as the attitude itself is concerned, it might well be designated *sensory thought* as may other experiences also, such as the reading aloud that was mentioned above. The memory cases also may go into the same category. The sensations, both in the memory instances and in those of the conscious attitudes, differ, if at all, only on their meaning side and not by the addition of any overt imagery. Already the reader may have surmised, and rightly, that the writer is introducing the theory that the representative function found in the raccoons and in the child, F, at least, who was most nearly comparable with them, is likewise *sensory thought* and essentially comparable with the cases above given.

Let us come to closer quarters with this theory. The proposed hypothesis is equivalent to making the so-called "imageless thought" genetically prior to "thoughts with images" and to placing the point of origin at least as low as the raccoon. As opposed to this, current discussion by advocates of imageless thinking would seem to assume the opposite, viz., that "thoughts with images" are prior.[52] It is to be noted that the assumption has no factual basis, but that it seems to be the result of prejudice or of temperamental leaning. My usage here takes for granted that imageless thought can be analyzed into either sensory or imaginal content which carries the meaning. It is the former case that I use as the type found as low as the raccoons. This thought is image*less*, though not sensationless, in the strict sense. In the light of this, I can see no intrinsic objection to the above theory, while the following points are in its favor: (1) The evidence in support of the possession of sensations by animals is absolutely convincing to any save one who denies all consciousness to animals. On the other hand, the evidence for images is very meager and unsatisfactory. Indeed, it is even more so that has been thought if, as I believe to be true, the present theory of *sensory thought* will account for all of the controlled behavior that has been used to support a theory of images where that behavior is not open to a stimulus and response explanation. (2) Raccoons and young children

[52] Bühler is an illustration of this. Referred to by Jas. R. Angell, *Psych. Rev.*, vol. 18, p. 317.

are capable of reactions that seem explicable only on the assumption of the functional efficiency of a representative factor. If sensations *can* function in this manner, the law of parsimony forbids the assumption of images. (3) That sensations can function in this manner is indicated by the illustrations from human psychology noted above. It is a different matter, I know, to say that the sensations of animals also may function in this manner. But in order to explain the behavior of the raccoons, it is necessary to assume either the presence of images or the presence of sensations that function in this manner. The law of parsimony favors the latter type of process because sensations are genetically earlier than images.

Inasmuch as the rats and dogs, as well as the raccoons, used sensory processes in the solution of our problem, we are forced to recognize that sensations may be placed in two classes on the basis of function: (1) There are sensations that can initiate a correct reaction from at least three possible responses *only when they have not been displaced*, during the interval of delay, *by other sensations of the same modality*. Here would fall those sensory experiences of constant orientation possessed by the dogs and rats. (Those cases where these animals lost their orientation and then regained it were few enough to be rated as accidental.) (2) There are sensations that can initiate correct responses, under the above conditions, even though they have been displaced during the interval of delay by others of the same modality. Here would fall the sensory experiences of the raccoons and children. The evidence which indicates that no sensory process need be constantly maintained in order that these subjects may react correctly is unambiguous. It is this type of sensory process that I have denominated *sensory thought*.

On the basis of human *introspection*, there is another grade or kind of learning, viz., the stage of the functional efficiency of images or centrally aroused conscious processes. Why there should be both sensory and imaginal thought in human experience is very difficult to say. The most obvious suggestion would be that imaginal thought, since it is genetically later, could accomplish tasks which sensory thought could not. I shall hazard no guesses as to what such tasks might be. At the present stage of psychological theory, there seems to be no question but that the distinction of sensory and imaginal thought

itself is valid. Future work must attempt the formulation of the functional differences between them.

The result of the above theoretical considerations is a classification of the grades of animal learning different from those hitherto advanced. Washburn,[53] e.g., has presented three classes: "First, there is the condition where, so far as we can see, the animal does not learn by individual experience. * * * In the next place, ve have the grade where the animal learns by experience, without having the power to recall an image of its experience. * * * Finally, * * * we have the possibility of an image." I would modify this scheme by the insertion of a stage called *sensory thought* between the second and third stages. The table would then read: (1) The animal shows an absence of learning by experience. (2) The animal is able to profit by experience, but has no higher capacity than "trial and error" or the "stimulus and response" behavior would indicate. The rats and dogs of the present tests come here. (3) The animal can learn by the "trial and error" method. *Indeed, probably most of its reactions are on this basis,* being ruled by stimulus and response. But a new element now makes its appearance, viz., *sensory thought,* which is a representative function of strictly sensory content. The raccoons that I used are in this class. Of the children, F is the most likely to belong to this class. (4) The fourth grade reveals the presence both of "stimulus and response" behavior and of sensory thought, but added to these is the possibility of directing reactions by images. The older children of the present tests very probably belong here rather than in the third class.

VII. SUMMARY AND CONCLUSIONS

The following is a statement of the results and conclusions that have been reached as a result of the foregoing experiments and analyses:

1. The rats (one excepted), dogs, raccoons and children made successful reactions in situations where the customary determining stimulus was absent at the moment of response. The stimulus might appear in any one of three boxes. These boxes were qualitatively alike, but situated in different directions

[53] Op. cit., p. 276.

from the release box. At every trial, three possibilities of reaction confronted the subject. A selection had to be made and that box chosen in which the stimulus had appeared most recently.

2. The conditions under which the maximal delay was tested and the results obtained are indicated as follows:

(a) Different classes of subjects were used. Table XIV gives the maximum and minimum delays that were obtained from the different classes.

TABLE XIV

Subjects	Min. delay	Max. delay
Rats	either no learning or 3rd stage of delay	10 secs.
Dogs	2 secs.	5 mins.
Raccoons	3 secs.	25 secs.
Children	50 secs.	25 mins.

(b) Backgrounds of widely different grades of brightness did not affect the intervals of delay.

(c) The use of a large release which gave the animals the freedom of the interior of the box lengthened the intervals of delay in the case of some subjects.

(d) The use of two boxes as opposed to three lengthened the intervals of delay by increasing the accuracy of response.

(e) Neither punishment nor the particular number of trials per day appear to have affected the interval of delay.

3. An analysis of the possible cues that may have been used by the subjects in the solution of the present problem gave the following results: (a) Overt orienting attitudes were the probable cues for many reactions of the raccoons. These attitudes must be assumed as cues for the rats and dogs in order to explain their reactions. (b) Some intra-organic (non-orientation) factor not visible to the experimenter must be assumed in order to explain a significant number of the correct reactions of the raccoons and all of the successful reactions of the children. These cues fulfilled an ideational function. (c) All of the reagents were influenced by external stimuli that were constantly present from trial to trial, e.g., those given by the box itself. However, these could not be used as a basis for selective responses inasmuch as they were constant from trial to trial and hence could not furnish varying, or alternating, cues.

4. No animal that had used overt motor attitudes in solving the problem when the small release and similar backgrounds were used adopted another type of cue either when a large release or when backgrounds of different brightnesses were used.

5. The method used in the present tests for attacking the question of the functional presence of a representative factor in animal behavior is superior to that of imitation, use of tools and others that have been used in the past, because here it is possible to determine what stimulus controls the behavior. It is therefore possible to insure the absence of the stimulus at the moment of response.

6. The representative factor for which search has been instituted in this monograph stands primarily for "objects" and not movements. A technique that would make certain a control of the latter factor so as to insure its presence or absence at the will of the experimenter has not as yet been perfected.

7. From a consideration of the theoretical advantages to be derived from interpreting this representative factor as sensory rather than as imaginal, a decision was reached in favor of the former alternative for all reagents save possibly the older children, H, Hd, M and L. Illustrations were given from human consciousness where a sensation performed a memory function or served as a link in a train of thought. Such cases have been termed "conscious attitudes" or "imageless thought." This function, as considered in this paper, was designated *sensory thought.*

8. The theory was advanced that such a function as sensory thought represents the highest grade of behavior in raccoons and probably also in children of some two and one-half years of age. This theory is supported by the hardly-to-be-doubted presence of sensations in animal consciousness and by the assumption that these sensations can function as the illustrations indicate that such processes do in human behavior. Such a theory seems more in accordance with the law of parsimony than would a theory which made images perform the representative function found in the raccoons and the child F.

9. From this theory, it follows that subjects may be put into at least four classes on the basis of the highest type of learning present in their behavior: (a) Absence of learning; (b) trial and error; (c) sensory thought, and (d) imaginal thought.

VIII. APPENDIX

A. Detailed Records of Two Rats and Two Raccoons.—The data given in the following two tables are self-explanatory. They give the course of the delayed reaction tests as these were presented to the animals.

TABLE XV

RACCOONS

Bob			Jill		
Delays	Trials	%	Delays	Trials	%
1st stage	10	100	1st stage	15	100
2nd "	10	100	2nd "	75	97
3rd "	179	67	3rd "	75	97
Controls were not perfected until the last 100 trials. Of these 72% were correct. Of last 50 trials, 80% were correct.			1 sec.	150	77
			3rd stage	150	92
			1 sec.	150	84
3 sec.	20	80	2 "	75	50
3rd stage	5	100	1 "	30	40
1 sec.	7	28	3rd stage	120	87
3rd stage	4	50	1 sec.	155	83
1 sec.	10	30	2 "	105	86
learning	17	52	3 "	45	57
3rd stage	50	88	2 "	105	79
1 sec.	11	72	3 "	75	56
3 "	50	56	Backgrounds were used now:		
1 "	80	70	2 sec.	90	58
2 "	50	78	1 "	45	71
3 "	20	90	3rd stage	150	97
4 "	62	69	1 sec.	75	93
5 "	56	82	2 "	105	96
8 "	20	55	3 "	90	66
5 "	40	75	2 "	45	55
7 "	10	100	1 "	45	71
8 "	189	55	3rd stage	60	96
last	48	79	1 sec.	45	97
10 "	25	76	2 "	60	98
12 "	29	68	(backgrounds off for	30	100)
15 "	30	73	3 sec.	45	93
5 "	1	100	4 "	45	62
5–15 "	10	50	Learning with light on 5 sec.		
15 "	10	60		30	100
20 "	18	44	Learning with light on 1 min.		
2 "	3	33		25	80
20 "	10	20	Wire release now, 10 trials daily:		
2 "	3	33	Learning	80	95
5 "	9	55	2 sec.	50	98
10 "	13	46	4 "	50	60
5 "	4	50	2 "	100	89
7 "	10	33	last	50	96
A— 5 "	49	75	4 "	50	88
7 "	20	75	5 "	50	70
8 "	36	80	last	30	90
10 "	50	72	6 "	40	90
12 "	10	90	7 "	50	84
15 "	10	90	last	30	70
20 "	96	64	6 "	30	86
last	46	71	Small release again:		
			6 sec.	30 (dropped)	56

TABLE XV—Continued

RACCOONS

	Bob		
	Delays	Trials	%
	Two boxes, a and c, from now on:		
	20 sec.	20	95
	25 "	20	90
	30 "	10	90
	35 "	70	64
	25 "	10	40
B—	15 "	10	60
	5 "	10	33
	Boxes a and b now used to break up position habit.		
	5 sec.	60	30
	3rd stage	10	40
	Learning	295	97
	2nd stage	240	86
	last	51	93
	1 sec.	51	94
	2 "	50	96
	3 "	51	60
	2 "	50	66
	1 "	10	40
	3rd stage	51	86
	Learning c alone		
		21	14
	Learning all three		
C—		120	80
	last	100	100
	Learning with light on 5 sec., boxes b and c		
		30	86
	last	20	100
	Learning with light on 1 min.		
		30	90
	Wire release, 3 boxes:		
	Learning	50	100
	2 sec.	50	48
	Two boxes, b and c:		
	2 sec.	50	94

	Bob		
	Delays	Trials	%
	2nd stage	10	100
	3rd "	50	100
	1 sec.	30	100
	2 "	40	97
	3 "	20	100
	4 "	50	80
	5 "	100	85
	6 "	50	88
	7 "	100	83
	8 "	100	70
	7 "	100	81
	8 "	110	89
	9 "	80	81
	10 "	50	86
	11 "	80	73
	10 "	50	66
	Boxes b and c from now on:		
	10 sec.	40	100
	12 "	30	100
	15 "	50	96
	20 "	30	90
	25 "	50	86
	30 "	71	80
	35 "	101	70 or less
	4 "	30	96
	5 "	30	100
	8 "	10	100
	15 "	20	55
	8 "	50	86
	12 "	90	84
	20 "	80	70
	first (dropped)	50	76

TABLE XVI

RATS

Rat No. 4		
Delays	Trials	%
1st stage	5	100
2nd "	10	80
3rd "	30	50
2nd "	5	100
3rd "	15	40
2nd "	75	70
3rd "	50	60
2nd "	75	85
3rd "	25	92
1 sec.	30	60
3rd stage	25	88
1 sec.	25	52
Different backgrounds now:		
3rd stage	25	60
2nd "	60	80
3rd "	50	80
1 sec.	25	64
1½ "	25	48
1 sec.	35	68
1½ "	50	62
1 "	25	64
3rd stage	50	64
Old backgrounds now:		
3rd stage	65	61
2nd "	75	94
3rd "	60	66
Two boxes now, a and c:		
3rd stage	25	100
1 sec.	20	75
	(dead)	

Rat No. 16		
Delays	Trials	%
1st stage	50	96
2nd "	100	98
3rd "	110	83
last	50	76
2nd "	100	99
3rd "	50	100
1 sec.	40	50
3rd stage	20	60
2nd stage	100	99
3rd "	50	96
1 sec.	30	36
Different backgrounds now:		
3rd stage	20	30
2nd "	90	96
3rd "	50	58
2nd "	100	99
3rd "	50	48
2nd "	70	100
3rd "	50	68
Two boxes now, b and c.		
3rd stage	50	92
1 sec.	100	87
2 "	40	97
3 "	50	98
5 "	50	90
7 "	35	60
Series with wire release:		
Learning	80	80
2 sec.	70	78
3 "	50	88
4 "	40	92
5 "	80	78
6 "	60	76
7 "	50	90
9 "	40	82
10 "	30	50
	(dropped)	

B. Notes on Raccoons.—Davis[54] and Cole[55] have given an excellent description of the habits of raccoons in captivity. In the main my observations substantiate theirs. A few differences, however, may be noted. Davis lays particular stress upon one habit which his raccoons formed, viz., that of covering their excrement. As well known this is contrary to their behavior in the natural state. My four animals were confined in one cage, 10 x 10 x 14 feet, the floor of which was covered an inch or more deep in shavings. Yet in all the long months during which the animals lived there, they never formed the habit of covering their faeces. These were always voided along the walls of the cage, and not once have I found evidence of an attempt to cover them. Moreover both Mr. DeVry of the Lincoln Park Zoölogical Garden, Chicago, and Dr. Hornaday of the New York Zoölogical Garden give an unqualified "no" in answer to the following question: Will the tame raccoon bury or cover his excrement, if given the opportunity?

No trouble was found in adapting the animals to a rather monotonous diet of bread and milk, varied occasionally with raw meat. But the amount of the rations to be given was more difficult to determine. Throughout the spring, summer and into the winter, the raccoon will eat voraciously. But by the middle of January, unless strict precautions be taken, the animals will be so fat that they will refuse to work and will sleep almost continually. My animals were kept on a back porch which was not artificially heated, and which was but slightly, if any, warmer than out of doors. Whether they would have gone into a genuine state of hibernation had I not cut down their rations in the fall, I cannot say. However, in view particularly of Betty's behavior, I am inclined to think that this would have happened. From about the middle of January until late in April, my notebook indicates that Betty lacked a motive for working. Tests were made during this period in order: (1) To prevent forgetting of the problem, and (2) to give the animal food in order that it might not become too weak from a long fast. The cases of the three other raccoons are not so extreme as this one,—yet all become less eager for food during the last

[54] Davis, H. B. The Raccoon: A Study in Animal Intelligence. *Amer. Jour. Psych*, vol. 18, 1907.

[55] Cole, L. W. Observations of the Senses and Instincts of the Raccoon. *Jour. of Animal Behavior*, vol. 2, 1912.

two months of the winter. Davis found that all of his raccoons hibernated during the first winter when they were kept out doors. But during the second winter when they were kept within doors, although unsupplied with artificial heat, they did not hibernate. How he regulated the food supply at this period, and whether the animals became sluggish, he does not state. Further observations should be put on record before the conclusion is reached that the raccoon will change so fundamental an instinct upon so slight a change of habitat. Both Mr. DeVry and Dr. Hornaday inform me that their raccoons do not hibernate in the winter, although the living quarters are no warmer than outdoors. In addition, Dr. Hornaday replies as follows in answer to the question of how to prevent hibernation: "By constant feeding. Bears and raccoons hibernate because they cannot find food in the deep snow. Our bears never hibernate because they are constantly fed." I, too, fed my animals regularly, although sparingly, in the fall, yet indications of a desire to hibernate were observed. The subject of hibernation is very poorly understood at the present time, even among the biologists proper.

Both of the authorities above quoted inform me that raccoons reach maturity at three years of age. I note this fact because Cole's statement that "The year-old raccoons apparently are not quite full grown"[56] may be as misleading to some as it was to me.

Two other observations may be noted in passing: (1) The raccoon appears to have a very acute vision. I have seen several individuals chase flies that were crawling upon the floor of the experimental room whose illumination was extremely low. (2) The price of keeping tame raccoons is eternal vigilance. In the spring when the "wanderlust" strikes them, they will gnaw wood and tear wire,—anything to escape. And the usual reward for attempting to catch a loose raccoon is a severe bite.

[56] Op. cit., p. 213.

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CONTENTS

INTRODUCTION. Page

Conditions for experimentation 1
Characterization of animals used 2

BIOLOGICAL AND NATURALISTIC STUDY.

Distribution 3
Description 4
Seasonal habits 4
Habitat and feeding 6
Viability, reproduction and development 6
Play 9
Vocalization 10
Quilling 11
Hands and feet 11
Naturalists and the porcupine 12

EXPERIMENTAL DATA.

Manual dexterity 15
Reactions to the puzzle-box situations 27
Form study 42
Discrimination of color and brightness 53
Discrimination of color following experiments with brightness 59
Threading maze 64
Rotation of maze 73
Dark-room maze experiments 76
Memory tests 77

RESUMÉ.

Conclusion 82

BIBLIOGRAPHY 83

Conditions for Experimentation.—In this study, experiments with the porcupines, from May 26th to October 20th, were carried on in an open-wire cage 8 feet by 20 feet and 7 feet high, situated on a gentle eastern slope under the trees at the experiment station connected with Clark University. On October 21st transfer was made to winter-quarters where work was continued until December 23rd of the same year. After that an intermission of 100 days occurred for the purpose of testing memory without intervening work. Various new check experiments were instituted when work was resumed in the spring. Winter-quarters consisted of a large, well-lighted inside room, with a ground floor, provided with dark dens in close approximation of those in the wild. There was also an outside run 9 feet square and 7 feet high. Adjoining the inside apartment were other rooms of a similar kind, where special apparatus could be used and where the animals could be isolated during experimentation. No artificial heat was used for obvious reasons. From May 26th to September 25th, experiments were made in the forenoon daily with a few short interruptions. After that it was found to be more convenient to do work in the afternoon. There was no difficulty, however, in changing the time of day or the place of experiment, though both changes were made with seven animals. Davis (14), it will be remembered, lost ten weeks time in changing his raccoons from outside cages to finished rooms and artificial heat. This difference in the behavior of the raccoons and the porcupines in respect to environment is very marked and the explanation is not certain. It may be that the porcupines are less sensitive to environment, which fact in turn, may be only a condition of acuteness of sense organs. It is also possible that porcupines can adapt themselves to a new situation more readily. And again, it may be that the artificial heat and the closer confinement of the raccoons lowered their physical vitality for a time. Still other equally obvious explanations might be offered.

The selection of this particular animal was made on the recommendation of Dr. James P. Porter, Dean of Clark College.

The insight which he showed will be more and more evident as the discussion proceeds. In all, 16 animals have been used as subjects for the more controlled parts of this study. Eight of these were male, four were female, and in the case of four the sex was not determined. In the following brief characterization where age is given it represents approximate age at the time of capture—somewhat definite in the case of younger animals but very indefinite in other cases.

Nos. 1 and 2. Sex unknown, large, brown. Received May 26th. Escaped after eight days preliminary observation. No experiments were made.

No. 3. Male, age three years or more, dark brown. Received May 26th, escaped June 23rd, recaptured in a tree near the cage. Escaped July 3rd, traveled nearly a mile the first night. Recaptured after three days in a tree near where he spent the first night. This was the most serviceable animal, —the one most frequently referred to in experiments reported below. Died April 29th the following year after all experiments were finished.

No. 4. Male, age about 14 months, light gray in color. Received May 26th, died August 15th as result of exposure in a cold rain. Served in maze and puzzle-box experiments.

No. 5. Male, age about three months, nearly black. Received August 1st, died August 12th. Used only in preliminary work. This was the youngest animal under observation.

No. 6. Male, age unknown, old. Legs, face and tail of this animal were nearly white, and long, bristle-like hairs tipped with white extended beyond the quills over the entire body. Captured by the author September 20th at the mouth of the dens by means of a snare on a fishing pole. Died January 15th. Autopsy showed hemorrhage of the lungs. Served in maze and form discrimination.

No. 7. Male, age unknown, mature; color, mottled dark brown. Very slow in movements. Captured by means of a snare in the natural runways September 18th. Very easily tamed but not very apt in learning on account of what I take to be erratic attention and slovenly habits. Died after all experiments were completed. Served in form discrimination and puzzle-box tests.

No. 8. Female, age about six months. Marked like No. 6. Captured in runways from the same den on the same night. Died October 30th. Cause unknown. Used in variations of hand reactions.

No. 9. Sex unknown. Age about six months. Dark brown in color. Captured in the same manner as No. 6 and on the same evening. Died in February, having lived in captivity more than 16 months. Served in variations of hand reactions and habit-breaking of brightness reactions.

No. 10. Sex unknown. Age about six months. Color nearly black. Received September 14th from New Hampshire. Very wild and never became thoroughly tame, but did good work nevertheless. Served mainly in color and brightness tests.

No. 11. Male, age unknown—old. Captured feeding in an oak tree in mid-afternoon, September 13th. Lost one hind leg in cage in some unknown manner. Used tail in walking as a substitute for leg. Died October 26th from effects of a fall in consequence of trying to catch a support with his missing foot while making a difficult passage on a shelf. Served in puzzle-box experiments.

No. 12. Old, male, medium in size. Nearly black with white quills on top of head. Captured by the author at 2 o'clock A. M. on April 7th as the animal was returning from the feeding grounds to the dens. Died April 17th. Cause unknown. Used only in preliminary hand reactions.

No. 13. Female, age about 12 months, small. Captured April 7th, after an all-night vigil at the foot of the tree into which she had gone to feed at 8:30 the evening before. Died May 11th. Cause unknown. Used in preliminary hand-reactions.

No. 14. Old, female, small. No peculiar markings. Received from Vermont, April 8th. Died April 25th from failure to give birth to young.

No. 15. Very old, female, large, very light brown. Received from Vermont, April 8th. Died May 2nd after artificial parturition of young.

No. 16. Old, male, very large, nearly black with a prominent crest of long, white tipped hairs. Received from Vermont with Nos. 14 and 15. Died April 17th. Cause, congestion of the bowels completely stopping the alimentary canal below the caecum. This animal was probably afflicted in this manner when captured, but as he tamed easily was used in preliminary hand reactions.

In presenting the porcupine to students of psychology as a subject for study in animal intelligence, a word should be said in introduction. So far as the writer knows, the animals which have here been under observation and experimentation are the only ones of this species which have been employed in experimental study. Nothing is clearer throughout the study than that a knowledge of the undisturbed life habits is indispensable to the evaluation of results obtained under controlled conditions, however close those conditions may conform to the natural environment. Consequently, a brief report is presented of a rather extensive study into the natural habits of the porcupines. The writer is indebted, in a way, to certain naturalists for observations, but most of the facts which are given, especially those of chief interest to psychologists, have either been obtained at first hand, or have been corroborated by a personal study of the porcupines undisturbed in their natural haunts.

BIOLOGICAL AND NATURALISTIC STUDY

Distribution.—The Canada porcupine (*Erethizon dorsatus*), commonly but erroneously called "hedgehog," is one of the largest native rodents of North America. It ranges in habitat as far north as timber is abundant. Formerly it was found as far south as Virginia and Kentucky, but has, for the most part, been exterminated below central Pennsylvania. The range west extends to the main ridge of the Rocky Mountains, sometimes reaching southward in the Pacific highlands even into Mexico.

Little is known of the geological history. The ancestry of the porcupine family has not been traced back farther than the Pleistocene age; hence, practically nothing is known of the history of the group. Remains of a species (*Hystrix venustus*) have been found in the Pliocene of Nebraska, but it is probably more closely related to the Old World porcupine than to the American genus. There are also a few Tertiary South American genera, but nothing has been made out directly connecting them with the Erethizon genus.

Description.—The weight of the adult porcupine varies from 12 pounds to 25 pounds, possibly larger. In appearance they are low, heavy-set with strong fore legs and stronger hind legs. The tail, slightly prehensile, is very strong, covered on the bottom with stiff bristles and on the top and lateral edges with heavy, short quills. The body is roughly conical, enlarging from the flattened nose to the very broad hips, making the animal well adapted to its habits of creeping around through the low undergrowth of the woods. In color they have great variation, ranging from pure white to a very dark brown or black. The brown and the black porcupines are very common, while the pure albinos are comparatively rare. The upper part of the body of the adult porcupine is covered with quills from the eyes and forehead to the tip of the tail, while the under part has soft, short fur which becomes coarser and longer on the sides next to the quills. Along the neck and shoulders these quills are longer, more flexible and very thickly set. On the rear half of the body the quills are shorter, very much heavier and not so numerous, leaving the skin well exposed when the quills are erect.

Seasonal Habits.—In its habits of life, the porcupine is nocturnal and perennial. On the first point there is little dispute. They usually come out an hour or so after dusk and are usually back in the dens an hour or two before dawn, thus avoiding both the daylight and the twilight enemies. In spite of their nocturnal habits they may frequently be seen roving about in the daytime for no apparent reason. The writer has made numerous and somewhat continuous observations throughout the winter months, however, to correct the error found in much of the literature to the effect that porcupines hibernate. It is

important to note that, though the captured animals were in natural temperature and had dark, cozy dens with an outside run where snow was abundant, experimentation continued until December 23rd with no appreciable diminution in their activity. In addition it was noted that throughout the remaining part of the winter their activity was in no way lessened even by a temperature of —15° F. Furthermore, a visit to the dens in the wild, when the temperature was far below zero and the snow 36 inches deep, showed the trails broken and the feeding grounds being used by them regularly.

Humidity has been found far more potent than temperature in determining their activity. It became a regular custom during this study to predict rain whenever the animals in captivity were observed to be more active than usual, although they can hardly serve as a standard barometer as certain writers have suggested. It has also just as frequently happened that they were quiet on a dull, rainy day. These observations are all the more interesting in that these phenomena have been fairly constant and simultaneous with from five to seven animals and in that they have been more marked in newly captured animals, where the natural instincts of food-getting have been least disturbed. In addition, it was noted that while capturing animals in the month of September, there was a close correlation between barometric disturbances and the number of snares which were thrown, probably, though not certainly, by the porcupines. In explanation, it should be said that the animal's exposure in the trees while feeding, as well as in the grass and weeds while going to and from the feeding grounds, taken with the fact that his coat is not impervious to water, would, no doubt, select and fix a tendency to feed as the barometric pressure was lowering. Similar observations have been made on hogs building a bed before a storm and on deer coming down from the hills when the warm south winds begin to blow. It would be interesting to know whether such an instinctive or habitual motor response is brought about by humidity or whether the stimulus is an olfactory one. Are the odors of the forest different in moist air? Does the presence of water vapor in the air lower the limen of olfaction? Such questions are hardly answerable for people, but might well be considered with animals where the sense of smell may be more acute.

Habitat and Feeding.—The dens of the porcupine are in the crevices of rocky ledges. It is impossible to observe them in the dens, but if one may judge from the habits of animals in captivity, they select their accustomed cranny, and their own private corner of that, roll up in a manner peculiar to the species, appearing almost spherical, with the weight of their body on the hind feet and tail, chiefly on the tail, with the fore arms dropped between the arched knees, and with the nose nestling between the fore paws. In this position they sleep throughout the day with scarcely a movement, except such as any sleeping animal will make to relieve strain and promote circulation. In districts where denning is less favorable they spend most of their time in the trees, feeding at night and remaining in the hollow trees or rolled up on a limb during the day; hence, the German name "*Baumstachelschwein*," or "*Kletterstachelschwein*."

Their natural food consists chiefly of the soft inner bark, leaves, twigs, etc., of trees. They feed chiefly on poplar, hemlock, fir, juniper and slippery elm. Early in the fall it is the custom of the porcupines to limit their feeding grounds to some specific cluster of trees. This results in the beating down of regular paths between the feeding grounds and the dens. For example, on September 2nd there was but one trail leading from a certain well colonized den which the writer had under observation. Two weeks later there were four of these paths. Soon after, a new one was marked out and all showed the results of regular use. Throughout the following winter, the animals followed these trails, approximating them even through the deepest snows. When spring came they abandoned the trails and fed again promiscuously over the forest with no definite routes for leaving the dens or returning to them.

In captivity they eat vegetables of most kinds but prefer cabbage, carrots and, most of all, sweet potatoes. Several animals learned to eat corn bread and refused all other kinds of food when that was obtainable. They would usually eat one kind of food for weeks or even months at a time, a fact, no doubt, bearing some relation to the tendency of the animal to return so persistently to the same group of trees for feeding in the wild.

Viability, Reproduction and Development.—The age, or span of life of the porcupine is not known; but from slowness of repro-

duction, usually one each year after the second year, one would judge it to be a long series of years. Shiras' (36) flashlight records of an albino for six consecutive summers is the only account of prolonged study that has been published. Porcupines do not thrive well in the cage and have never been known to breed in captivity. The former circumstance is difficult to explain. Each of the animals in this study which died has apparently succumbed to a different malady so that no generalizations have been possible. Dr. Hornaday, writing of the Canada porcupine in the guide to New York Zoological Garden, May 1st, 1907, says: "It is only the men who know all about animals who can tell us why nothing seems to satisfy them, and why they will not breed here, live 10 years and be happy." But even these men have not found the remedy.

Concerning the fact that they will not breed in captivity only one explanation is suggested. If the report of hunters and woodsmen who have witnessed the act can be credited, copulation is a very complex process,—so complex indeed, as to involve submission on the part of the female more complete than is ordinarily found among higher vertebrates. During sexual congress, the female is said to lie on her back with the male directly above her,—a submission hardly probable under the strained and limited conditions of cage life. The writer does not vouch for these facts, although there are identical reports of eye-witnesses on various occasions and there seems to be no other physiological possibility than for the bodily contact during the sexual embrace to be on parts of the body where there are no quills. The position and structure of the sex organs are entirely consistent with this report from the woodsmen. There seems to be no means by which the male could capture or hold the female during copulation.

The young of the porcupine have never been studied and have probably never been seen alive. Observations in this study are limited to three infant porcupines, two of which were taken dead from the body of the mothers which had died before undergoing labour, and a third taken dead from the mother after a failure at labour, the mother dying a few days later. One of these young may be described as follows: Length of body was six inches, and of tail two inches. Whole outer surface was covered with rather coarse hair, with no quills, but

with roughened skin where the quills would probably have first appeared. Claws were hard and perfectly formed, rudimentary thumb not appearing even in a callous scar. The soles of the feet were warty as in the adult. The bones were hard but not completely calcified. The teeth appeared only as soft buds or swollen places on the gums. Sex organs were dimly discernible. The external ear was perfectly formed and open into the tympanum. The eye slits and nasal passages were open. Vibrissae were wanting, or were very slightly differentiated. This individual's development may not have been normal, but it is more probable under normal than above normal.

There were some marked differences in the case of the other two infants. They were somewhat larger and longer. The vibrissae were grown. The teeth were through and fully enameled. What is most significant is that they were covered with very hard, sharp, barbed quills from the point of the shoulders to the top of the head and from the middle of the back to the tip of the tail. There was a region along the back of about one and one-half inches where there were no quills. Whether this shows any atavistic tendencies is not certain since the geological history is not well known. The high development of the foetus taken with the fact that the mammary glands of the mothers showed no signs of swelling preparatory to giving suck, suggests that the Indian belief referred to by Brehm (5) may have some foundation of fact. In speaking of the "Unk Wunk," the Indian legend says: "The mother (porcupine) has no teats, furthermore the young cannot suck, and consequently the mother is accustomed to drive the young from her immediately after their birth and thus force them to begin their hard gnawing work from the first day of their lives."

The story may or may not be founded on fact, but these young porcupines dying before actual parturition were physiologically equipped for climbing and gnawing, from the day they should have been born. The question at once arises, if this early independence is as evident as the legend and as the physiological maturity would suggest, how will this affect the psychic life of the animal? Will the porcupine prove to be like the guinea pig with its great precocity and its limited ability at maturity, or like the rat which is helpless and almost senseless at birth but which shows a high degree of plasticity in later

life? Will the porcupine conform to or depart from the law of direct ratio of length of infancy and degree of plasticity?

The limitations which circumstances have thrown around direct knowledge of the infant porcupine and consequently upon exact judgments concerning it are fully appreciated. One can do little more than state the problem at this time. Even to anticipate a whole series of results by saying that the porcupine has not shown indications of the arrest which characterizes animals of short infancy, but has proved very adaptable under widely varied circumstances and through a great range of ages, there is presented possibly only a wide variation from a very general rule. This later plasticity is evident. The short period of infancy is strongly suspected, but we must wait at least until the living infant has been seen by some one, until it has been observed critically and subjected to tests as other animals have been, until its nervous system at birth has been examined, before concluding positively on the conformity or non-conformity of the porcupine to the general rule of the significance of infancy. It need only be added that if the period of gestation is seven months, as Brehm claims and as is the case with the Old World species, some of the infantile stages may be included in the long prenatal state.

Play.—No one has ever seen a porcupine play. In fact, play seems so foreign to the porcupine's nature that it would be a matter for surprise to see one of them turn a trick in pure amusement. They frequently quarrel, rarely slap each other, more rarely still seize each other by the quills, but an encounter is seldom seen in which it does not appear that each participant would rather the contest had not occurred. This, however, is an anthropomorphic view-point given for the purpose of making clear the habits of the porcupine. It is not considered safe to infer that any animal is playing when it makes many quick movements, nor that slow movements indicate a serious type of mentality. It is imperative for the porcupine's survival that they move slowly except when hard pressed by an enemy. The maximum of pleasure might well go along with slow movements and a minimum of danger. On the other hand, the quick, restless spontaneity of a young animal or even a child may well represent a more serious mental disposition than adult human beings ordinarily suspect. The term play is used in its broad sense. In this popular sense, porcupines do not play.

Vocalizations.—The most common sound in the vocalization of the porcupines is the bark, or yap with a tonal quality not unlike that of the human voice. It is used during their quarrels and seems to express a state of anger and resentment. The same quality of tone made nasal and drawn out into a whine is used when an animal is being robbed of its food, or driven from its corner in the den. It seems to have the same quality of emotional setting as the first, less the element of active resistance. The third vocalization is also nasal but unmistakably different from the second. It is rounded, mellow and crooning, with a quality very hard to imitate and even more difficult to describe. It is said to be used freely during the mating season and is certainly more common in the season when the young are born. It has been mistaken a few times by experienced observers for the cooing of doves, and might be so confused by one less acquainted with the porcupine. It seems to be the one conciliating factor in the social control of the porcupine.

Somewhat related to these vocalizations and more common than all of them combined is the chatter of the teeth. Such an action on the part of the wood-chuck seems intended especially to terrorize and is accompanied by starts and charges; but in the porcupine the act is not so highly specialized. The wood-chuck will even bite an antagonist and the unfleshing and chattering of teeth may well be considered anticipatory to that form of defense. Porcupines do not defend themselves in this manner. The reaction seems to have a less definitized mental accompaniment and in case of fright may persist for 20 or 30 minutes after the occasion would seem to be passed. Such a reaction may be called out by a distant noise where there could be no chance of terrorizing the enemy. The failure of a piece of apparatus to act in the ordinary way is frequently "scolded" in this manner. The porcupine may chatter when the apple given him is a little too sour or the carrot has a spoiled end. In addition, it appears to be a signal of suspected danger, very similar to the stamp of the rabbit's foot for, when once started, it is often taken up by others in what would seem to be a reflex if not an imitative manner. The writer is quite sure that the fact of his presence, or rather the presence of danger, was, on several occasions, communicated by means of this telegraphic code to all in the dens, spoiling the observation of the evening,

save for this one fact. In general, it is held that the chattering of the teeth is an expression of mild fear, general disappointment and sensory displeasure, and a very general signal of uncertain danger. The above general conclusions must not be interpreted too broadly. The writer, really, is in accord with Davis (14) in assuming that "interpretation of vocalizations * * * is possible only in terms of accompanying circumstances."

Quilling.—Contrary to a very wide spread opinion, the porcupine cannot "shoot" its quills any more than a bird can throw its feathers or a human being eject the hairs from his head. If the belief were less widely spread, even among educated people, it would be superfluous to speak of it here. The only thing the porcupine can do with its quills is to erect them over its entire body to some extent but chiefly on the rump and hips. To express this reaction it has been found necessary to ascribe an intransitive meaning to the verb "to quill." The Century Dictionary uses the verb "to porcupine" in this sense on the authority of Wolcot (Peter Pender), but it does not appear to be a happy selection. When in this discussion it is said that the porcupine "quills" the reference is to the same type of reaction as the "bristling" of the boar, the "fuzzing" of the cat, the "fluffing" of the cock, and possibly also to the indescribable tremor which passes through the human skin in case of shock or fright, a reflex, thought to be vestigial of what was very prominent in some less human forbears. A porcupine can strike a powerful blow with its tail, can hurl its whole body at an enemy by turning very quickly and can lurch heavily upward when attacked. All these movements are very quick and may bring the quills on the porcupine in contact with the flesh of the enemy. Owing to the fact that the outer points of the quills are barbed, all such quills cling to the victim more firmly than they normally do to the porcupine's body and are thus given off from the porcupine by contact. This leads to the belief that the quills are hurled at the enemy from a distance—an illusion somewhat pardonable when one appreciates the quickness of the porcupine's movements when warding off an attack.

Special Characters of the Hand and Foot.—The forefoot of the porcupine is freely used as a hand and is well formed for that

adaptation. Throughout this discussion the forefeet of the porcupine are referred to as hands, as they figure as such in the experiments. The human hand, it will be remembered, has three distinct lines, two transverse and one longitudinal. All of these lines are equally plain in the paw of the monkey and chimpanzee, and are well marked in the hand of the porcupine, though the digit corresponding to the human thumb is wanting in the latter case and the fingers move in unison with little or no independence of the digits. In place of the external thumb there is a knot or ball very much as would appear if the human thumb were amputated at the second joint. An examination of the skeleton shows a rudimentary toe or thumb perfectly articulated and armed with a claw. In one instance the hardened claw was protruding half its length. The whole is beneath the surface in most instances with only a cartilaginous scar showing through the epidermis. This hardened, rounded ball serves the animal very well in grasping and is more advantageous than a thumb would be in walking. The hand is limited in movements to the simultaneous flexing of the fingers and the rotation through about 90 degrees by means of the wrist and forearm. This rotation is exactly the adaptation necessary for the porcupine in its change from walking on a horizontal surface to climbing the perpendicular trunk of a tree.

The hind feet have five claws and very broad flat soles with little of the possibility of movement seen in the hand. They are adapted best for walking, or standing and sitting in an erect posture.

The Naturalists and the Porcupine.—Even the more careful naturalists have honestly misjudged the behavior of the porcupine in many ways. In his study of the quadrupeds of North America, Audubon (3) finds the Canada porcupine the slowest of them all. Burroughs (6) uses this sluggishness to account for both the physical weakness and the stupidity of the porcupine. Bearing on the first point he cites the instance in which he struck one of them on the nose with a rotten stick merely for the purpose of confusing it, killing it with a blow which he thinks a wood-chuck or a coon would hardly have regarded at all. Then he argues: "Thus does the easy passive mode of defence of the porcupine not only dull its wits but makes frail and brittle the thread of its life. He has no struggles or battles

to harden or toughen him. That blunt nose of his is as tender as a baby's and he is snuffed out by a blow that would hardly bewilder for a moment any other forest animal." In his reference to the passive resistance of the porcupines Burroughs is not exact as was indicated above. The porcupine does not remain quiet and allow the enemy to charge and impale itself on the quills. When the porcupine is expecting an attack, it is tensed in every muscle. The legs are stiffened. The body is raised. The tail is held up from the ground. The head is bent low. If the attack is to be from the front the porcupine waits till the enemy is near, whirls with a quickness which the eye can scarcely follow, strikes a telling blow with the tail and at the same time lurches heavily upward with the back, thus vanquishing his enemy with one stroke.

Speaking of animal intelligence in general, Burroughs says: "Our common porcupine, for instance, has little use for wit or celerity of movement. It carries a death dealing armor to protect it from its enemies and it can climb the nearest hemlock tree and live on the bark all winter" (op. cit. p. 3) This credits porcupines with far more viability than they possess, and disregards the "tender as a baby" attributes referred to above. Again Burroughs says of the porcupine: "He is as stupid and as indifferent as the skunk; his broad, blunt nose points a witless head" (op cit. p. 58). These expressions are but typical of the reactions of the better naturalists toward the "porcupig" as they often call it. One would also suspect on Mosso's (30) hypothesis that its exceedingly slow movements would beget a correspondingly dull mentality. Experimental results will either determine whether the charge of stupidity has any justification or whether our laboratory methods really test the intelligence of animals.

The writer's own experience with the animal previous to this study was limited to one encounter in the mountains of Colorado in mid-winter several years ago. There he reacted in the customary way by clubbing the creature to death with the same feeling of satisfaction one would experience in killing a snake or a spider. Even on the arrival of the first consignment used in this study there was an unexpressed wish that the porcupine had more of the graceful cunning of the raccoon or fox, the neat attractiveness of the white rat or chip-munk, the

eccentricities of the dancing mouse, the sociability of the cat and dog, or the intellectual cast of the monkeys and higher primates. In later experience, however, considerable effort has been necessary to keep attachment to the animals from influencing the evaluation of results. Nevertheless, all emotional bias has been avoided even to the extent of forcing interpretations into lower categories than the necessity of the case demanded in an effort to give the facts a maximum scientific value.

Such are the chief characteristics of the animal here presented for a subject for experimental study of its learning process. Porcupines have been charged with unbounded stupidity by naturalists and pronounced ugly and repulsive by most who know them. Save for a few references which depict it as the victim of persecuted innocence and stupidity, the porcupine has seldom figured in the aboriginal folk-stories which eulogize sublety, cunning, sagacity,—in general, intelligence of animals. It probably has a very short infancy, is comparatively playless, and has strong solitary tendencies. It is very easily tamed, and is devoid of any great amount of curiosity which Davis (14) considers an "essential symptom" of intelligence and without which he asserts "animal intelligence is out of the question." Not a single observation in this study can be interpreted as due to "spontaneous attention and the instinct to investigate." How will this animal react to the ordinary methods of the laboratory for testing intelligence? The greatest assets of the experimenter are the porcupine's appetite and the persistency while on duty. Little difficulty is experienced in the way of diminished effort or activity as the animals approach satiety during the feeding. They usually stop suddenly and return to their corner after having eaten the last morsel almost as greedily as they did the first. Barring the case of No. 7, which for a time presented the problems of the "backward child," all of the animals used in this study have been most persistent workers. There has been no tedious waiting for the animal to get ready to attack the problem provided its solution was between him and the food. Although they are somewhat slow, they seldom pause in their activity, almost never give up a task, and work with an independence of effort which leads one to suspect that their survival has depended upon the individual far more than upon the group.

EXPERIMENTAL DATA

Manual Dexterity.—In the process of feeding and taming the animals when they were first captured the problem of right-handedness and lefthandedness presented itself as a study of unexplored possibility. Kinnaman (26) found his monkeys giving preference to one hand over the other, particularly in certain acts, and inferred that this was due to the fact that monkeys stood relatively high in the scale of development. Davis (14) thought he found the same tendency in his raccoons and uses the same assumption, based also on Burke's theory of development in children and Féré's theory of degeneration in imbeciles, for arguing that the raccoon is above the average in the scale of intelligence. Cole (10) failed to report any tendency of his raccoons to use one hand in preference to the other. None of these experimenters, however, made any tests. At this point no attempt will be made to rank the porcupines; but a few results of tests on the procupine's use of his hands will be presented. In all cases except that of No. 4, which will be explained later, tests were made immediately after capturing the animals, before any manual predispositions were brought about by cage environment. The animal in the natural state is free to use his hands in pulling down branches since he can support himself on a perpendicular trunk of a small tree, with his hind feet and tail, using the bristles on the bottom of the latter as a clutch. The forearms and body are then free to be extended in various directions, even directly away from the support.

Porcupines Nos. 3 to 16 inclusive, excepting No. 11, were used in these tests. No. 11 was not used for the reason that he had but one hind leg and it was feared that this fact had thrown the forearms out of symmetry. A detailed account of the reactions of No. 3 will be given and the others presented in groups as checks upon his results. For the tests, the animal was induced to mount a box about eight inches high, and to stand, or to sit tripod fashion on his hind feet and tail near the edge of the box so that nothing could interfere with the free use of his hands. This is a very natural position for the porcupines and is, therefore, no distraction. It soon became a regular habit with No. 3 to mount the box at feeding time so that the task of getting him placed for the test was not disturbing. The experimenter would then take a position in front of the animal

Food was handed to the animal in small pieces from the experimenter's right hand, care being taken to place it always in the same manner, as nearly as possible, directly before the animal's nose. Another precaution was taken to expose food equally before both eyes so as to neutralize any tendency to monocular vision which might predispose the porcupine to follow sight in one eye with the hand on the same side. The results of the first 11 days' experiments are shown in table I. These data were recorded in detail, but, as the individual records show little more than the totals for several days, details are omitted. It is sufficient to say that the animal's changes from right hand to left hand and back again were made promiscuously throughout the tests, but were grouped more towards the beginnings of the first feeding periods. By this means the over-night lapse of the habit was observable, but does not appear in the table.

TABLE I

PORCUPINE NO. 3. FREE REACTIONS WITH HANDS

	Right	Left	% Right
1st day	23	25	47.92
2nd "	64	23	73.57
3rd "	41	13	75.43
4th "	103	26	79.85
5th "	291	13	95.79
6th "	253	8	96.94
7th "	300	5	98.36
8th "	311	1	99.68
9th "	420	0	100.00
10th "	300	0	100.00
11th "	476	0	100.00
Total	2562	114	

As is clearly evident, the table is a reliable measure of the rate of progress in the taming of the animal. Even more clearly do the tabulated data show a tendency of the porcupine to become right handed. After the fifth day the only responses with the left hand came in isolated cases in which the animal failed to secure food with the right hand, or when he dropped the food before getting it into his mouth. This might appear at first to be a determination on his part to try with the other if he failed with the first hand. His voracious appetite and his impatience when the feeding rhythm was interrupted would justify the supposition that any mentality of which he was

capable was exercised. Close observation, however, showed that when food was seized with the right hand it was transferred to the left in the act of cramming it into the mouth, leaving the right hand free to reach for the next morsel. Thus the two hands were used alternately when all went regularly; but when the right hand failed to provide food for the left to pass into the mouth, the action of the left hand which habitually followed that of the right hand had nothing to express itself upon. It was, therefore, the hand which, on the principle of the reflex arc, one would expect the animal to employ in getting his next particle of food. When this hand received the food and carried it to the mouth, it performed the feeding act without transfer to, or assistance from, the right hand. In this way the normal rhythm was restored which was for the right hand to be extended for the food after the left had finished putting the last portion into the mouth. And thus the irregular behavior is explainable as a simple reflex—the perfect working of a well formed habit.

No. 4 in the first test was fed from the experimenter's right hand and also developed a righthandedness, after showing considerable tendency to be ambidextrous. Soon afterward, this animal became lame in one hind foot and could not stand well, so work was discontinued even before the process had been perfected, but not until there was no doubt of the direction in which he was tending. Two months later, after No. 4 had spent his time on puzzle-box and maze work, with no intervening hand training, he was started again. He was believed to be free from any set tendency with his hands due to the few days' earlier experience. But this time the experimenter presented food with his left hand. The animal showed the same tendency to use both hands but delevoped a lefthandedness, using the right hand alone only once in 600 times. No. 6 was fed from the experimenter's left hand and varied the procedure by becoming righthanded, using the left hand only once in 406 trials. No. 9 was fed from the experimenter's left hand. He began taking the food with the left hand, used the right hand on the 18th reaction, and not again till the 79th with only five lefthanded reactions in 700. No. 8 was fed from the experimenter's right hand and used the left hand only twice in the first 20, twice in the sixth 20, and no more in 625 reactions. No. 7 was fed from the experimenter's left hand and became

lefthanded with only one variation in 600 tests. No. 10 made six variations in the first 30 but no more in 470 tests. In the case of Nos. 12 and 16 the experimenter used the right hand, and each became righthanded with only a few errors at first. Nos. 13, 14 and 15 were each fed from the experimenter's left hand and all responded with their left hand after a few irregular reactions. No. 15 showed less tendency to settle down to the use of one hand than any of the others in the later experiments. This may be due to her extreme age and long experience establishing the bilateral habit. Personally the writer has tested 10 animals and No. 6 is the only one which failed to react with the hand corresponding to the one with which the food was presented. One would suppose that if No. 6 had had anything like the struggle which No. 3 experienced, he might have formed a different habit. Some other factor seemed to have determined No. 6's reaction. An assistant was allowed to take four newly captured animals and check the work under the same precautions which had been observed. In all 14 animals have been tested. In seven cases feeding was done with the experimenter's right hand and in six of them the animals became righthanded. Of the seven instances in which food was presented with the left hand, six developed lefthandedness. The two odd cases out of the 14 may well be treated as exceptions.

Individual differences in the animal's progress were not very great under any given set of conditions. Nos. 4 and 5 belong to the earlier tests and conform more closely to No. 3 as shown in tables I and II. This is attributed, not so much to the fact that these animals were slower, but rather to the fact that the experimenters were so fearful of getting their fingers bitten that they did not keep the conditions constant. Considerable dexterity was required to place the food in the animal's hand so that he would not drop it and lose the reward after he had made his reaction. The effort on the experimenter's part was to make every reaction a successful one for the animal. As this skill of the experimenter operated wholly after the porcupine had reacted, it is considered an environmental condition merely distracting, if not controlled, but of no consequence as a cue to the animal previous to his reaction.

As to the bearing of all these results on the general question of bilateral symmetry and its noteworthy exception in man's

predominating righthanded tendency, many conjectures might be made. Alsberg (2), Baldwin (4), and Mollison (28), along with others, hold that this human trait is due to the fact that people are left brained which is, in turn, attributed to the anatomical advantage of the left carotid artery over the right, thus giving the left hemisphere more and better blood at a higher pressure. The fact that the habit once formed is scarcely ever broken argues for some advantage which makes it different from most other habits. Baldwin (4) postulates that it must be due to inherited physiological superiority in the quantity, quality or arrangement of the brain material of one hemisphere over the other. The original cause of the differentiation which has resulted in righthandedness he thinks must be due to spontaneous variation. Livi (27), the distinguished Italian anthropometrist, has advanced the theory that righthandedness and lefthandedness are due to "uterine position." He shows that in fully 90% of the cases the left hand and arm of the foetal body are cramped under so as to inhibit prenatal movement while the right arm is above, swings free, and is thus more easily exercised and consequently becomes more fully developed, at the same time most probably inducing greater development of the hemisphere of the brain in control. As this is exactly the percentage of righthandedness among people according to Buschan (7) it appears a very strong probability. I have nowhere seen the conjecture that the prenatal position may be a *result* of unsymmetrical development in arm or brain in which case the stronger member would extricate itself and leave the weaker member to sustain the weight of the foetal body thrown over on that side. If this seems to be as reasonable as the other it only serves to remove the problem one stage farther, leaving it still unaccounted for except on the basis of innate physiological superiority. Buschan (7) accepts the theory of Livi (27) with a few slight additions, chief among which is the fact that the right breast of the mother is larger and stronger, thus causing the human infant to be held more frequently with its left arm pinioned against the body and its right hand free during a large part of the waking life of earliest infancy, and especially during the feeding period where psycho-motor activity is most intense and steady, or where the greatest rewards of activity are being secured.

Gould (15) explains the whole problem upon a different basis: "All that is needed to explain the righthandedness of 94% of the children is some ancestral savage custom, habit, or necessity widely prevalent which inclined to the use of the right hand and eye for one or two exceptionally intellectual tasks. The inheritance of aptitude, the force of custom and the necessity of the struggle for existence would certainly fix the persistence of the peculiar excellence." The same author also proposes another explanation: "physiologically, therefore, the reason why an infant puts forth the right hand to grasp objects is because the right eye is the one which is nearest perfect visually, anatomically or optically. The law derived from the phylum of the entire past is that the right eye and the right forefoot, or right hand, must work together * * * Handedness, if one may devise a word, becomes either righthandedness or lefthandedness, according to the dictating condition of the better eyedness, right or left." In spite of Baldwin's assurance that it is impossible and Gould's dogmatic assertion that it is nonsense that animals are right footed or leftfooted because the differentiation "could only arise with sign language and counting, and animals do not make gestures or count," Mollison (28) has demonstrated that all the primates at birth show characteristic anatomical differences in the fore limbs which correspond remarkably with the asymmetry of the species.

Porcupines have very little, if any tendency to be either righthanded or lefthanded, as shown in the facts given below that habits in the use of the hand when once formed could be broken in a few days with few traces of the old habit remaining, and to return to one of them was a task sometimes greater than first forming or breaking the habit. But the reason why they responded symmetrically with the experimenter is not to be explained upon a physiological basis. If it were due to suggestion from the operator it was not only unconscious but contrary to the conscious efforts to prevent any such cue. If there was such an influence it was sufficiently general to permit of change of experimenters in the midst of the learning process in 10 different animals without disturbing the learning curve. This was exactly the case with the well-known horse "Hans" as well as with Haggerty's dog, but still it seems hardly reasonable when one considers what slight changes in condition

will disturb the porcupines, provided the modification in any way affects the cue by which they are learning. It would be dogmatic to say that the experimenter has not made the porcupines use their right hands when he used his right hand and their left hands when he presented food with his left hand, but if this has been the case while at the same time it was being consciously guarded against, then the same thing, with the inhibitions thrown off, and conscious encouragement added, as would almost universally be operative in the case of the human infant, is sufficient to account for the perpetuation of right-handedness once it is established as a human trait. The persistence of the habit when formed in the human infant has never been tested as rigidly as it has been in the animal. This will be shown in the next paragraphs. The child of seven months is no fit subject for experimentation sufficiently intensive and extensive to break a motor habit of this character. It would be more reasonable to conclude that in the case of the porcupines there is a sense of balance with the environment, or, maybe, a mere convenience which places the right hand in the *line of grasp* of the right hand of the experimenter, and the animal responds to that situation neither more nor less mechanically than children do. There is a certain naturalness about it all, which, in the initial stage, tips the balance toward symmetry with the environment without involving any large degree of mentality and without presupposing any anatomical superiority of bone, muscle, eye, or nerve.*

At this stage the experiment was modified, and the records of No. 3 will again be reported in detail. In the preliminary work cabbage was fed altogether. Food was now changed to carrot, cut in small pieces, and at the same time the porcupine was forced to take it with his left hand, food being refused when he extended his right hand though the experimenter still

* Since the above was written the author has been interested in reading the study "The Retina and Righthandedness" by Stevens and Ducasse. *Psy. Rev.*, Vol. XIX, No. 1, Jan., 1912. Those writers made experimental investigations of the relative size of the perceived image on different areas of the retina. Their results indicate that the "space sense" of the left side of the retina is greater than that of the right side. They conclude as follows: "Those objects in the right half of the field of vision, by appearing larger, attract the visual attention, which in turn leads to grasping movements with the right hand." The theory would be more readily accepted in this causal relation if it were known that this disparity of the space sense was true of earliest infancy and was not itself induced by the child's environment.

presented the food with his own right hand. In table II the plus sign indicates that the animal was correct in reaching with the left hand, while the minus sign indicates errors in reaching with the right hand. The tests were made in series of 20 without any appreciable pause between tests or series.

TABLE II

− − − − + − − + − + − + + − − + − + − −	7	correct	35%
+ − + − + − + + + − + + − − + + + + + +	14	"	70%
+ − + + + + − + − − + + + − + + + + − +	14	"	70%
− + + − + − + − + + + + + + + + + + + +	16	"	80%
+ + + + + + + + + + + + + − + + + + + +	19	"	95%

Nothing could be clearer than the progress with which adaptation is made to the new demands. It was a simple case of solving the problem of how to obtain food. In 2024 tests No. 3 made 54 errors, 30 of which were made in the first 100 as shown in the table.

When the animal had attained perfection in this line the experiment was narrowed down to the real problem of teaching him to reach for cabbage with his right hand and for carrot with his left hand, although the experimenter still presented food to him with the right hand in the same manner each time, and with each kind of food. Table III gives detailed results of the experiment.

TABLE III

Date	1 Length of ser.	2 No. of chg's.	3 Err. on change		4 Err. within ser.		5 Total error	6 Total tests
			rt–lt	lt–rt	rt.	lt.		
June 26.....	100	4	1	2	20	5	28	400
" 26.....	50	4	1	2	14	1	18	200
" 27.....	50	2	0	0	2	1	3	100
" 27.....	25	26	3	3	19	6	31	650
" 28.....	25	30	4	2	22	7	35	750
" 29.....	25	30	2	3	5	3	13	750
" 30.....	10	70	2	5	7	2	16	700
July 1.....	10	70	0	3	5	2	10	700
" 2.....	Irreg.	120	3	0	2	1	6	700

Table III shows: (1) The number of morsels of each kind of food given the animal before changing; (2) the number of changes made each day; (3) the failure of the animal to change hands right to left and left to right with change of the food stimulus from cabbage to carrot and carrot to cabbage; (4) the errors which he made in changing his hands when there was no change in the food stimulus; (5) the total number of errors; and (6) the total number of tests each day.

At first No. 3 was fed 100 morsels of cabbage and then 100 morsels of carrot, but as this length of series was great enough to reestablish the habit each time the number was dropped to 50 before a change, then to 25 and to 10 in a series, and lastly with no definite number, ranging from three to eight before a change of food was made. All of this is clearly indicated in the table. At first, before starting again after a change, he was given a piece of the food of the kind to be used and was not allowed to use his hands in taking it. Later this was discontinued. The result of the entire practice was that after about 350 such changes of food and hands involving nearly 5000 tests and 160 errors No. 3 reached for cabbage with his right hand whenever, wherever, and by whomsoever it was offered. He was considered perfect when he made the irregular changes 100 times in succession without an error. Thirty days later with no practice with his hands in the meantime, he showed little or no loss of ability to make the discrimination. The 100-day memory test yielded results indicative of almost perfect power of retention. His dislike for cabbage at that time, however, made results rather indefinite.

Nos. 4 to 10 inclusive except No. 6 were used as direct checks upon this and showed equal skill. Little would be added by a detailed account other than what is given in the modified checks given below. One of the most convincing arguments that the experimenter did not present the food in a different manner and thus suggest the change of hands each time is seen in the conduct of the animal when mistakes were made as to how some particular animal had been trained. Experimenters occasionally recorded a dozen or more errors in the beginning of a day's work and then stopped to learn the cause of the trouble, only to discover that the animal was reacting according to his habit at the time they were expecting the opposite and recording errors. This could not be voluntarily tested as that would have destroyed the condition necessary for its operation as a check. Three times, however, without arousing suspicion, the writer intentionally gave wrong directions to an assistant. Without exception, the complaint was soon made that something must be wrong as the porcupine was reacting in just the opposite way. As this little deception was not divulged until all experiments were completed, the writer is convinced that sufficient

checks have been employed showing the amount of the unconscious influence which the experimenter may have had on the animal's responses.

The conclusion is that the animal's basis for reaction was something other than the unconscious idiosyncracies of the experimenter. It is thus different from Herr von Osten's horse, and the experimenter has successfully eliminated himself. How the porcupine makes this discrimination between foods cannot be said with absolute certainty. At first it was possible for them to use taste. Smell was always possible though neither of them would furnish reliable criteria to the animal. The fact that the discrimination was made and the hand brought into service while the mouth was yet full of the last morsel and the senses of taste and smell were flooded with its effect, leads the writer to believe that these senses could not have been reliable guides. Vision was, no doubt, the sense depended upon. But whether they used form, size, brightness or color is not certain and not easy to determine.

In the control experiments now to be described, animals 7 to 10 were used, sweet potato being substituted for cabbage. The same conditions exclude taste and smell from the possible sense stimuli. Form and size were eliminated by cutting the sweet potato and carrot into pieces of the same general sizes and shapes. Anticipating some later results it may be said also that color can safely be dropped from the consideration, leaving brightness as the most probable and possibly the only criterion for discrimination. It seems clear that the seven porcupines which have acquired this ability give good evidence of voluntary discrimination followed by a definite muscular response. The word "voluntary" is used to characterize the reactions particularly significant on the numerous occasions when the food was changed and the animal would start to reach with the hand previously used, in an automatic, or reflex manner. On discovering the changes he inhibited the responses half executed, and reacted at once with the other hand. The experimenter is again sure that there was no involuntary start on his part to cause the animal to inhibit one reaction and change to another. The measure of choice may not be as definite as in Powlow's (51) salivary reflex method with the dogs, but it has the advantage of using voluntary muscles instead of

unconscious reflex glandular activity. While the latter may test the senses more accurately it does not seem proper to use it as a criterion of the learning process. The two cases stand apart on the ground of the voluntary and involuntary features of the reaction. The writer is not aware that any experiments have been made with animals that are comparable with those just described with the porcupines. The problem has been carefully worked and nearly 30,000 individual records have been considered in the foregoing discussion.

If one would rank animals on the basis of manual dexterity it would count for far more if they should pick up articles with the hands and perform acts such as grasping and carrying food to the mouth. This is a regular habit with the primates and was noted frequently by Cole (10) and Davis (14) in the raccoon. It is not the natural habit of the porcupine to use his hands in this way unless the food is out of reach, or is so placed that it can be reached with the mouth with difficulty. Food is first grasped and raised with the teeth and then held and manipulated with the hands. In the experiments just described, involving the use of the hands, food was readily transferred to the mouth when it was placed in the hands. But even after such a habit had been well formed the animals fed in their natural manner off the ground, seizing food first with the teeth. An attempt was made with Nos. 8 and 9 to determine the rate with which these animals could be induced to make use of the hands instead of the mouth in lifting food. No. 8 had shown very remarkable adaptation in the other tests and here she fell into the habit very readily. A piece of food was reached to her on a stick and she very quickly grasped it and took it off. After some practice in this the food was fastened to the stick and held on the box in front of her. If she undertook to take the food with her mouth the food was removed. This was continued until she took it regularly with the hand. The food was then laid in front of her without the stick but was brushed away if she attempted to lift it with her mouth. This was continued until no precaution needed to be taken. The food was then given to her in bulk and she reached for it regularly with her hand. Later it was noted that in gathering scattered fragments from over the cage she picked them up in a very unporcupine manner and had, at least temporarily, lost the tendency to

use the mouth in lifting food. How long this would have persisted unfortunately can not be known, for No. 8 died soon after this part of the experiment was completed.

No. 9 acquired equal facility in this line but after much greater effort, and even then the habit was not exercised very freely in remote parts of the cage. No. 9 would also revert to the natural method of feeding the moment the experimenter's attention was diverted. This tendency to lapse to the more primitive type of behavior is seen whenever the animal's training does not conform to his natural habits. Even the celebrated chimpanzee "Peter" drops on all fours under intense excitement or when his keepers are not observing closely. The habit of picking up food with the hands did not persist in any marked degree with porcupine No. 9, although this animal employed the hands in other experiments more than is customary with the porcupines and occasionally grasped food found in the cage, raising it to the mouth with one hand. Whether this was a mere coincidence of individuality, or the result of a permanent modification of behavior, is uncertain, though the latter seems to be far more probable.

No. 9 was then set the task of handling a cup in one hand and taking food from it with the other hand. Porcupines do not need to be taught to hold a cup or box in both hands and take the food from it with the mouth, as they frequently seize the food-boxes in that manner if an opportunity is afforded. But this different use of the two hands involved a new co-ordination, one which is not seen in an untrained animal unless it be the same co-ordination necessary in holding down branches of trees and picking off fruit, but even here the mouth is used to pull food off and the free hand to assist in getting it into the mouth. The mental side of the association became well established but the muscular co-ordination was not so well perfected. The cup merely hung from the hand and was not held up as we hoped it would be. This may be accounted for either by the fact that the cup was unsuited to the hand, or the hand was not suited to this kind of adjustment.

More than six months later this same animal was again tested on the tendency to pick up food with the hands. There had been no analogous training during that time. Many times when food was offered the animal it had reached with its hands,

and hundreds of times it had reached into the food-boxes in the course of other experiments. In this 184-day memory test, the animal was induced to mount the same box previously used and food in small pieces was placed before it as in the original experiment, although a different food was then being employed. The porcupine at once seized the food with its hands and at no time attempted to grasp with the mouth first. On the following day the same cup which had been used before was placed before the animal. It at once grasped the handle of the cup with its right hand, lifted it from the box and took out a piece of food with the left hand. The performance was even better than at the time of the original practice. The animal remembered without any loss of association, or dexterity in performing—even with improved skill. This was far more than was expected as the special effort of the experiment was to train the animal contrary to its natural habits. When a light aluminum cup was substituted for the porcelain cup previously used it was held even better than in the original learning. In fact, the animal showed as much skill in handling the food-cup and taking food from it as one could expect. The question whether such training is sufficiently unique to be retained better is an interesting psychological problem. Or, had the animal really been trained along the line of its instinctive aptitudes? As to the latter it must be repeated that in all the observations made in connection with this study no untrained animal has been found performing any act closely analogous to these last variations of the hand reactions. If Yerkes (49) is right, however, in emphasizing the difficulty in training an animal contrary to its instinctive behavior, one would have expected much more difficulty in inducing porcupines to accommodate themselves to this modification not closely correlated with their natural habits, as well as poorer memory of the association and a rapid loss of skill in the performance of the act.

In regard to the use of the forefoot as a hand it may be said that the porcupine shows less dexterity than the monkeys and possibly less also than the raccoons though no adequate tests of monkeys and raccoons have been made.

Reactions to Puzzle-box Situations.—A few experiments with puzzle-boxes with various devices were carried through. The main purpose was to compare the porcupines with other animals

in such tests, but little more will be attempted than to present results, leaving the reader to make all direct comparisons desirable. Although Thorndike and Kinnaman have been the teachers of us all in this kind of work, the apparatus was modeled more exactly after that by Davis (14) as shown so admirably in his study of the raccoon. The box itself was 11x12x16 inches with a door six inches square in the middle of one of its sides next to the ground line. This door was a light frame covered with one-quarter inch mesh wire and was hinged at the bottom by the inner edge in such a manner that gravity sufficed to throw it open when the fastenings were released. The door was made of wire in order to expose the food and give the porcupine an added stimulus, but the result proved to be quite the contrary as it led the animals to spend time struggling for the exposed food, thus diverting their attention from the locking devices. This reaction was most prominent during the first efforts and it is probable that the character of the door was non-significant after the experiment had proceeded a little ways. The door was provided with "L" shaped projections on the inner edge into which the locks could be adjusted when it was closed. Locks and combinations used were as follows:

1. A simple push down lever was adjusted at the right-hand end, extending about six inches, working in a groove in the end of the box, and pivoted on the inner side of the front so that when the projecting end was raised a little above the horizontal, the door was fastened.

2. A plug was adjusted in the top slightly toward the front and right of the middle point. Attached by a string in the end of the plug was a concealed lock similar to the lever.

3. A horizontal thumb button four inches in length was placed at the upper left-hand corner and adjusted to turn upward.

4. A hook of the ordinary screen-door variety with the bend taken out of the point was placed vertically near the upper left-hand corner and made to open toward the left.

5. The above four were placed in combination so that they could not be operated except in the order named; i.e., enumerating in reverse order, the hook opened against the button and consequently must come after it, the button was held by an inside mechanism which was released only by pulling the plug, while the plug in turn, was fastened until the lever was

depressed. On the other hand, when the lever was down a spring would catch it so that it could not be raised; when the plug had been extracted releasing the next part of the apparatus it could not be reset; and when the button had been turned upward it could not be restored to a horizontal position. These last checks were not all used at any one time but were ready for use whenever the porcupine manifested any disposition to reset the devices once properly operated.

Porcupine No. 4 was given this problem first, but as the apparatus was new, the animal little understood and the experimenters inexperienced in such work, no attempt was made to obtain quantitative results. The chief value was that it trained the experimenters for taking more accurate records and observations in succeeding experiments. The procedure followed was to feed the animal at the entrance of the box occasionally closing the door until the animal began to look for the food, then opening it and attempting to build up an association between the food and the inside of the box even when the door was closed. After one day devoted to this preliminary drill the door was closed and fastened by the lever at the beginning of the feeding period. No. 4 tried to get at the exposed food directly, but soon went around to the left end and on the top of the box as if looking for another opening. On this excursion it was probably his constitutional sluggishness that prompted him to climb over the lever from the back rather than to walk around it. In doing so he depressed it, the door opened and he secured food 53 seconds after beginning. On the next trial he again scratched on the door for a time, then turned to the left around the box clawing and sniffing as before. The third trip was very similar but the fourth showed a distinct variation. This time he started toward the right, passed around the end of the lever and climbed over it from the back side, opening the box in 5 seconds. This procedure he continued until the 10th trial when, on his trip around the lever, he stopped, examined it from the front, probably attracted by some odor, then used both hands in pushing it down while he was yet in front of it. Probably, even in this case, he was preparing to gnaw the lever but on hearing the door open his attention was diverted. This was his ultimate solution. It was an excellent opportunity to explain by saying that the animal "knows where

the hitch do lie," but the olfactory stimulation and the fortuitous success in opening the door, later a fixed motor response, seem to offer a better explanation. Besides, this conforms with the general policy of refusing an explanation involving higher mental processes when one involving lower mental processes will suffice. No. 4 opened the box 40 times the first day and would have continued twice as long in all probability but, as yet, neither the full measure of the appetite nor the endurance of the porcupine had been appreciated by the experimenters.

An incident the second day following is worthy of mention. On the 152nd trial, No. 4 opened the box as usual, but, failing to find his food, went back and pushed on the lever again, though the door was open and the lever was down. After this he found his food. The three following trials he pushed the lever, turned, and although he could see and hear the door open, he returned and gave the lever a second push before looking for his food. After this he eliminated the second attack on the lever, restoring his usual habit. The influence of that one successful chance variation, however, came near throwing the animal into a new method of procedure. Had it been as simple as the former it might have persisted longer than it did.

The writer is in full accord with the efforts of those recent investigators who would eliminate the expression "trial and error" from the studies of animal intelligence. It is fundamentally wrong and can have little significance unless a definite negative stimulus is imposed, throwing the emphasis on the wrong rather than the right reaction. Normally, the animal learns by "trial and success." Erroneous movements pass without significance and tend to drop out and disappear. The important factor, in all laboratory work at least, is the successful movement. The same might also be said when the animal is in the presence of its enemies. The individual which escapes lives to engraph its success on the instincts of the species; while those which make the fatal error are exterminated and the species profits little by their experience except in a negative way. So long as No. 4 clawed at the door of the box, no progress was made; but as soon as the lever, or the end of the box where the lever was located, or even a certain trip came to be associated with the process of getting food, progress ensued very rapidly.

The same thing appeared even more clearly when another locking device was substituted for the lever. The plug gave No. 4 considerable difficulty and it was necessary to rub it with a piece of food to give it an odor in order to attract his attention to it. Up to this stage, where appeal was made to the olfactory sense, No. 4 had errors or failures in abundance, but what he needed was a success. Nor are the porcupines peculiar in this respect for other experimenters have experienced similar difficulties and have usually resorted to the same plan of solution. The necessity passes after a few trials in the case of most higher animals. After operating the plug five times the effort of porcupine No. 4 was directed toward the plug as a means to an end, without examining to see if it were good to eat. The button, which was the third device used, gave even more definite results. The sixth trial again found the animal with the problem solved. He operated the button by putting one hand on the top of one end and the other hand at the bottom of the other end, giving the button a quick, firm turn till the internal mechanism squeaked. How significant that sound was to No. 4 is only conjectural but he was considerably disconcerted on one or two occasions when the sound did not occur although the door of the box fell open as usual. The hook gave No. 4 considerable trouble. It was of iron and he persisted in opening it with his teeth. He also made the mistake, for some time, of pulling directly outward on it, but later learned to operate it very dextrously. In the case of the hook the third trial was a direct attack although his movements were not properly co-ordinated till much later. Thus an average of 6.25 ± 1.9 trials has enabled porcupine No. 4 to associate these devices with the opening of the box if one may infer from the first direct attack on the device then in use. This leads to the belief that the mental association of the porcupine, in a vague and general way at least, may often be far in advance of his ability to profit by experience, or, in other words, intelligence may be in advance of dexterity which is usually taken as the measure of intelligence. The same thing may be expressed in a less objectionable way by saying that it is easier to get attention directed to the proper sensory stimuli than it is to get that stimuli to discharge into the appropriate motor responses.

When No. 4 was given all these locking devices in combination series there was hopeless confusion during the first feeding period. The chief difficulty was that he neglected the lever. He had reverted to the lever position so many times during his troubles with new parts that he had broken this association. He succeeded in opening the box 14 times but still had no grasp upon the problem. The next day, as he did not begin with his customary vigor, the plug and hook were removed and the button turned vertical leaving him only the lever for 20 trials. He soon re-mastered the lever, eliminating habitual associations with other devices. When the plug was added there was another long, hard struggle to learn the two in order. The button and the hook were next added simultaneously and were soon mastered. Then followed a long series in which the box was opened successfully in much the same way each time. After pushing the lever down he would stretch up over the corner of the box and pull the plug. Then without getting down he would edge over and downward, following almost a direct line with his head from the plug to the button and hook. The time for the combination varied from seven to ten seconds.

Porcupine No. 11 was given this puzzle-box as his first problem after being captured. The number of tests was limited so that they could be tabulated. The records for each device represent the work of a single day, but the whole represents a little more than one week. No. 11 was allowed to open the box 40 times when 20 were not sufficient to fix the co-ordination on the muscular side in attaining dexterity; though it was often associated on the mental side much sooner. This, it will be observed in table V, was necessary in the case of the lever, button, lever-plug combination and in the whole combination.

Every experimenter appreciates how inadequately time by a stop-watch tells the story of the animal's behavior but the well established custom of presenting results in this one unit of measure has been followed. Table V needs no detailed interpretation.

The large numbers in the series of time records for the combination deserve a word of explanation as they indicate much more than really exists. It was the habit of No. 11 to open the button and the hook by reaching down from the top after he had mounted the box, in a manner similar to that of Davis'

raccoon No. 1. In operating the combination the porcupine occasionally omitted the plug or lever before mounting the box. In these instances he found that the button would not turn and he was compelled to climb down and start over. The fact that he had only three legs made him reluctant to do this and slow about it when he did. In saying this, the writer is

TABLE IV

Time Records of Porcupine No. 11 with the Puzzle-box

Lever	Plug	Button	Hook	Lever & Plug	Combination
80	315	147	67	241	315
30	17	206	1	39	39
64	15	171	14	87	283
24	4	157	8	230	185
19	6	73	15	16	140
12	23	145	13	97	310
10	26	25	10	46	120
10	15	17	7	10	60
20	13	30	7	98	21
5	3	14	10	43	15
9	3	17	9	21	67
3	1	12	12	7	122
3	1	30	10	40	80
3	1	15	5	10	50
5	1	27	5	6	414
1	1	10	5	32	48
3	1	10	7	4	222
1	1	23	6	5	130
1	1	8	6	5	29
3	1	12	6	8	42
1		6		4	37
8		7		3	40
3		7		4	56
3		7		4	23
5		5		4	14
2		6		5	32
4		6		16	16
2		9		3	30
1		9		5	21
3		6		3	12
3		7		3	18
2		7		3	24
1		6		4	20
1		10		3	15
1		7		4	15
1		8		4	13
1		12		5	16
1		6		3	11
1		7		3	35
1		7		4	9

The figures represent the number of seconds elapsing between the time the animal began to try to get the food as nearly as that could be judged, and the instant the door opened.

supposing that under normal conditions No. 11 would do as other animals have done, namely, begin anew when he found that any part could not be operated. Thus, the long time of any one effort usually means one small error plus a great inconvenience in rectifying it. The low record for the second effort on the hook was made by the animal striking the hook from below instead of climbing on top as was his custom before with the button and the first trial with the hook. This was one of the few instances where a successful reaction tending toward simplification did not materially modify the behavior of the animal.

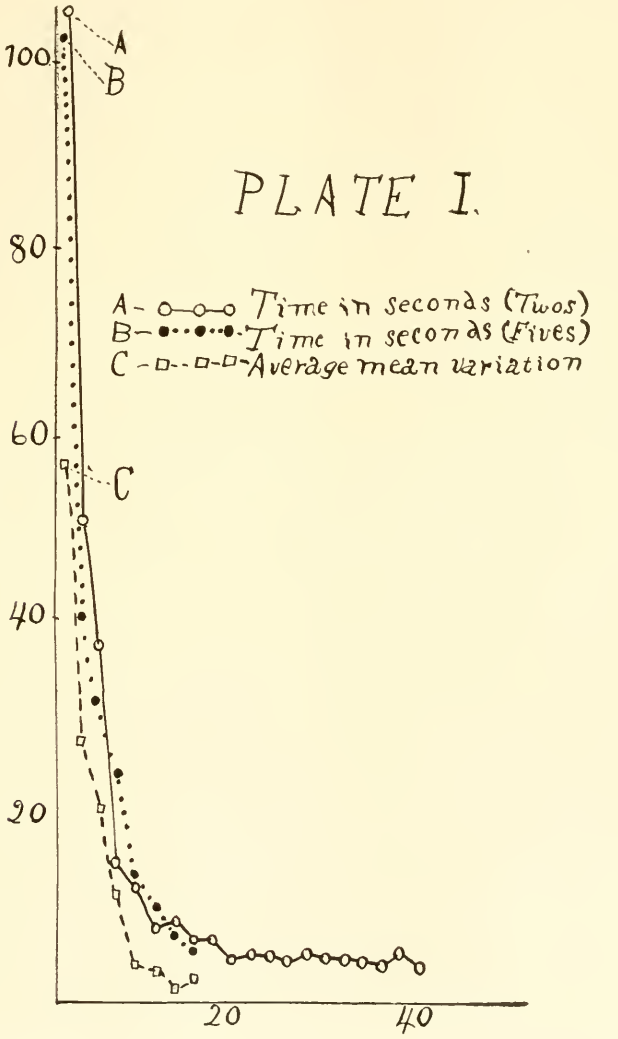
The records of the first four devices shown in table IV are represented by curve A in plate I. To obtain the curve the time records for the various devices were arranged successively and these results were grouped and rearranged by twos, taking the first of each series, second of each, etc. This may be taken as fairly representative of the learning curve of the porcupine under these particular conditions. Table V was derived from table IV in the following manner: The time records for each device and combination were arranged successively in groups of five and the results tabulated in the first six columns along

TABLE V

Lever	Plug	Button	Hook	Lever-Plug	Combination	Average of all
43.1±22.8	71.1±97.1	150.8±32.6	21.0±18.4	140.6±93.9	192.4± 85.2	103.2±58.4
11.4± 3.6	16.0± 6.8	46.6±37.5	9.4± 2.1	58.8±30.9	105.2± 87.8	41.2±28.1
4.6± 1.9	1.4± 0.6	20.2± 6.6	8.2± 2.5	16.8±10.9	146.6±106.9	32.9±21.6
1.8± 1.1	1.0± 1.0	12.6± 1.1	6.0± 0.4	10.8± 8.4	114.2± 53.3	24.3±11.2
4.0± 2.0		6.1± 0.7		3.8± 0.3	34.0± 12.4	12.5± 3.8
2.4± 0.8		7.2± 1.4		6.4± 3.8	22.4± 6.8	9.6± 3.2
1.6± 0.7		7.1± 1.4		3.1± 0.4	18.4± 2.8	7.8± 1.3
1.0± 0.0		8.0± 1.6		3.8± 0.6	16.8± 7.2	6.9± 2.3

The figures at the left in each column (table V) indicate the average number of seconds taken in groups of five which porcupine No. 11 used in operating the different devices and combinations. The figures at the right in each column show the mean variation for the same groups. The right-hand column combines the other six by averaging the two items separately along horizontal lines.

with the mean variation in each group. To obtain the column at the right, the other columns were combined horizontally by finding the average of the averages for the first, second, third, etc., groups of the first four devices and two combinations, together with the average mean variation corresponding to the same groups.



The slight increase in time, and particularly in the mean variation for the button and two combinations is, no doubt, due to partial satiety of the animal as he approached the close of the feeding period in each case. It should be noted that the column showing the average of all does not indicate an increase in time, and the average variation is only one second greater than the preceding group.

The last column of figures is represented graphically in plate I by curves B and C. Curve B shows the diminishing time used in operating various locking devices and combinations. Curve C shows the average mean variation. It is clear that improvement in the two respects goes on almost parallel. As the animal reduces the time necessary to operate any particular device, or set of devices, he also becomes more regular in his method and consequently shows less and less variation in the time required. The only instance where the mean variation is greater than the average is in the first five trials at the plug where the average time is 71.4 seconds and the mean variation is ± 97.4 seconds.

Little would be gained from combining curves of other animals with that of No. 11 to show a general learning curve of the porcupines for the reason that results are, in many respects not comparable. For the same reason it would be misleading to combine curves of other animals observed by other experimenters in an effort to get a still more generalized composite curve. Before such a combination would be at all illuminating certain very obvious conditions must be kept uniform.

1. Experimenters must agree on just what stage of the animal's attack shall be considered proper to start the time record. Shall it be when the animal first starts toward the box? Or, shall it be when he first attacks the particular device then in use?

2. There must be definite agreement as to when the animal has opened the box. Shall the experimenter say that when the door swings open the animal has succeeded, and disregard the two, three or even 20 seconds which he wastes in useless effort after the food is exposed? Or, shall the watch be stopped when the animal ceases to work with the device and directs his activity toward an open box rather than toward a closed one? How

much shall be allowed for variation in judgment of the experimenter in these cases?

3. The resistance of the different parts of the apparatus must be considered with respect to the size, weight, etc., of the animal so that each will have the same advantage in the way of "hair trigger" devices.

4. The amount of previous experience of the animal with other apparatus must be known and controlled.

5. The number of experiments in the particular test series which is to be used for comparison must be limited to some definite standard of excellence or to some specific number of reactions.

6. Similar precautions might be enumerated in respect to the particular devices applied and the order in which they come, the degree of hunger, the presence of the experimenter, the use of negative stimuli, the number of tests per day, etc., etc. Or, at least, all of these conditions should be kept constant throughout any one series of experiments.

In regard to the work with porcupine No. 11 the following regulations have been strictly observed:

1. The watch was started when the animal made the first pass at any part of the box.

2. The watch was stopped when the door came open regardless of when the animal took notice of it.

3. No "hair trigger" locks were used. Each device was made to work easily but with a constant friction through a definite distance.

4. No. 11 had been captured recently by the author and had had no previous experience in captivity, except a few days general taming and accommodation to cage environment.

5. All experiences with apparatus are accounted for in the tables and curves, as completely as time will represent them.

6. The usual hunger of the day was depended upon and after experiments had been completed the animal was allowed to finish eating the daily allowance of food.

7. No negative stimulus of any kind was employed.

8. By good fortune the animal in every case succeeded in mastering the new device in a single day, if not by 20 then by 40 efforts as the table indicates.

9. The experimenter was in the cage at all times with a definite position while the animal worked.

The writer is aware of the severe criticism which is being directed toward this custom and the oft repeated cautions concerning the "elimination of the experimenter." The points are well taken as special cases but are hardly to be considered general laws. If the writer is not mistaken, the controversy arose from, or at least was brought to a climax by the behavior of Haggerty's dog in his reactions to the area stimulation with and without the leash although the experimenter was attempting not to direct the dog's action by means of the leash. This, the writer recognizes as the most probable thing to expect with an animal bearing the close attachment to man which the dog does. One would expect them to be peculiarly sensitive to the wishes of the master. The same tendency would be true with the horse and was with the famous "Hans." It would be less true in the case of other animals unless the stimulus should arouse fear. In the case of a semi-solitary animal like the porcupine, where there is certainly no social relation with man, the presence of the experimenter would probably be less significant.

Porcupine No. 3 was used also with the puzzle-box. The method with him and his rate of learning was nearer that of No. 4 than of No. 11. Whether the rapidity of No. 11 was due to individual differences or difference in the method is not certain. It is probable that too much practice was given on the separate parts with Nos. 3 and 4. Too much experience served to fix the new habit so strongly that it was a hindrance to the forming of later associations. It would seem even with animals to be pedagogically wrong to give too extensive drill on any isolated step no matter how fundamental it may be. Or, put as a general proposition, there probably is a point of efficiency, different for each individual, where retention of the old and advance to the new is most easily accomplished. If this is not attained, the habit is effaced in the next series of associations. If it is exceeded, injurious inhibitions, even to the extent of arrested development may be set up and progress is more difficult. No. 3, however, mastered the devices and combinations very readily for the reason, if for no other, that he was a most persistent worker.

Memory or retention tests with the puzzle-box combinations were made on No. 3. After 13 days he had forgotten nothing and had lost none of his dexterity. He was allowed to open the box only twice at this time. At the end of 30 days he still attacked the devices in proper order but had lost a little of his dexterity in operating the button. His method of turning the button was rather awkward at best. He would reach over from the left of the door across the top of the button with the left hand, inserting his claws between the end of the button and the hook. In this memory test he would reach half an inch too far and grasp the hook. His dexterity, however, was all regained after one successful co-ordination. Fifty days later, or at the end of 80 days after first learning the combination, he had lost the response to the lever from his reactions. He would regularly go back and look at the lever but always began his attack with the plug, following with a trial of the button and hook in order. When all failed he would again walk around to the end, look at the lever, turn away and attack the plug. After repeated failures of this kind all the devices were removed except the lever. He at once opened the box. After a few experiences with this, the combination was restored and he opened the box with little difficulty. How may we explain his attitude toward the lever? His conduct was the same as that on former occasions when he returned to the lever after he had already depressed it. He would not attack it again on those occasions but merely inspect it and pass on. In this memory test the lever position at least was in the association, but he seemed to mistake the height at which the lever ought to stand or, put in motor terms, the sight of the lever failed to call out the proper motor response and thus the whole series of movements was interfered with.

It may also be interesting to relate that it was after relearning on this occasion that No. 3 was used in demonstration before the seminary at President Hall's house. Here, in a new environment and artificial heat, in the night-time, on a library table, and surrounded by a large crowd, the porcupine opened this puzzle-box combination repeatedly in a manner that was above reproach. His special preparation for this was to be practiced three evenings in artificial light, two of which were in artificial heat and two were on the table. His appetite was

sharpened for this occasion by giving him only half his customary allowance of food the day before. The writer is not aware that any other animals, subjects of experiments by psychologists, have been as successfully employed for class demonstration as the porcupine. All except one of the animals in this study have shown similar docility and composure under cage conditions. Others may decide whether it indicates a high or low order of intelligence, or whether it indicates anything so general as that.

The 100-day memory test showed much better retention than the 50-day test. The time for the first operation was 1 minute and 31 seconds and for the second 20 seconds. Trials 3 to 10 were somewhat irregular but all due to his failure to operate locks sufficiently when he had attacked them. The average time for the 10 operations was 28 ± 13 seconds which, nevertheless, taken with the behavior of the animal, gave four experienced observers the impression that the porcupine had retained the association well but had lost chiefly in dexterity. There are several obvious explanations for the superior memory of the 100-day test over the former 50-day test. (1) The 50-day test was really 80 days after first perfecting the association. He had been tested with the apparatus twice at 13 days and 10 times at 30 days. (2) The second relearning for purposes of class demonstration may have had special significance as to the importance of repetition in facilitating retention. (3) It must also be remembered that during all of those 80 days, No. 3 was busy with other lines of experimentation, while during the last 100 days no experiments were attempted. This would indicate that the conditions of normal cage life were not so distracting as the controlled problems of experimental work. This corroborates the finding of other experimenters with various other animals where memory has been tested during periods of activity and corresponding periods of inactivity.

In training porcupine No. 7 to operate this box a variation was made in the method. This plan was to add the new parts in succession but to retain the first ones in combination as they were learned. The plan would probably have worked well had the animal not refused to pull the plug. Even when it was coated with food he would carefully gnaw off the food and leave the plug set. Day after day for nearly two weeks he taxed the

patience of the experimenters. The lever was removed, the plug was made smaller, then shorter, then longer. It was saturated with food and the animal was starved for 48 hours to induce him to put forth greater effort; but still he did not attack it in the proper manner. Everything the animal did resulted in failure so he became indolent and presented all the pedagogical problems of the "dull individual." The plug was wrapped in a cloth to make it soft but he would not do the one thing necessary: viz., take hold of the plug with his teeth and pull. At length, on the suggestion of Dr. Burnham, a carrot was hollowed out and fitted down over the plug in such a way that if the animal tried to take the carrot off the box to eat it he would in that manner pull the plug. This brought the first real success. He bit the carrot and pulled the plug, the door opened striking the hind foot of the animal. This so startled him that he released the carrot and then noticed that the door was open. The next effort resulted in the same experience. On the third trial he had shifted his position so as to avoid the fall of the door and this time he attempted to eat the carrot without pulling the plug. On the third day following, however, he had mastered the problem as far as the carrot-plug was concerned. When this carrot was removed he again refused to pull the plug. The change was too great. The carrot was restored but gradually trimmed smaller and smaller till on the 10th trial there was only a small piece tied on the side of the plug with a string. This seemed to be sufficient but the animal would always seize the piece of carrot rather than the plug. On the 20th trial there was only a small piece tied on the side of the plug but he refused to pull it. The following day he was again started with a small piece tied on the plug but after opening the box eight times the piece was removed and this time the animal seized the plug and pulled it so hard that he strained the internal mechanism. After that there was no more difficulty. The lever which had been removed was restored and the button and hook added in regular order. so that in a few days No. 7 was as expert as other animals had been and like the latter opened the box when placed on the top of a table in the presence of several visitors.

In comparing the habits of the porcupine in opening puzzle-boxes with that of other animals used in that kind of experi-

mentation, one fundamental difference should be noted. The attention of the monkey is easily distracted; the cat seldom takes any problem seriously; the dog is always ready to do his master's bidding rather than to solve a problem of his own; the birds are afraid to abandon themselves to the task; rats, mice, and guinea pigs are each handicapped in their own way; and so it might be continued. With the porcupine there are few, if any, of these disturbing factors. One can depend upon the porcupine's best effort most of the time. More than that, he goes about getting his dinner with an abandon which would suggest that he had not an enemy in the world and had never depended on anyone but himself for his survival.

Conclusions from Puzzle-box Experiments.—

1. Porcupines do not show very great individual difference in methods of attack on any new lock or device, but each evolves a characteristic way of operating it after a few trials.

2. Any fortuitous movement which proves successful usually persists and is gradually reduced to its simplest form.

3. When a habit is once formed by the porcupine it tends to persist unless an accidental variation happens to be in the direction of simplification.

4. In so far as tests have been made the porcupines have shown an ability to form associations between sense stimuli and motor response which is quite comparable with that of other animals.

5. Retention of such a problem as the puzzle-box shows that associations on the mental side, or at least in the more general adaptations, persist more perfectly than muscular co-ordinations. Or, more concretely, on memory tests the animal directs his efforts to the proper devices in the proper order far more successfully than he co-ordinates his movements in operating those devices.

Form Study.—The apparatus to test the porcupine's ability to discriminate form was designed to appeal directly to the denning proclivities of the animals. Eight blocks of planed lumber nine inches square and one inch thick, approximately symmetrical and similar, were used. Out of the middle of the blocks the desired forms were cut with a fine saw. The form used, however, was the opening through the block rather than

the portion taken out. A circle seven inches in diameter was taken from one. A square 6.2 inches was cut from another. For the hexagon a side 3.8 inches was used. An equilateral triangle having the same area as the circle was found to present to the human eye a very common illusion of being much larger. It was also found that the nine-inch block would not contain a triangle of that size so a triangle with a side 8.2 inches was used. This compensated for the illusion to the human eye and allowed it to fall in the same general dimensions of the other figures. A rectangle and an ellipse were also made approximately the same area as the other forms.

These forms were presented to the animals pair-wise and six-wise by means of frames made to hold the forms of an angle of about 75° from the horizontal, with the base of the form 10 inches above the ground. Back of the forms were stationary food-boxes separated from one another by solid partitions and the boxes were set in and down far enough to prevent the animal from seeing the food before he put his head through the form. In addition, a secret pocket was made in each compartment where a piece of the kind of food being used was concealed at the beginning of each day's work. The purpose of this later precaution was to eliminate the olfactory sense as a factor in the selection by distributing the food odor uniformly over the apparatus. To eliminate the possibility of the animal's trailing himself and thus finding the food, the forms were regularly turned so as to present a new base line and reversed to present a new surface surrounding the form. By this means, the circle and the square could each have eight symmetrical positions. The hexagon had two on each face with an angle at the base and two with a side at the base. The other figures did not lend themselves so well but were manipulated with every possible precaution to prevent the animal from using a cue other than form as a means of selection. Special precaution was taken when the animal was nearing success but there was no indication that secondary criteria were employed. All the form boards were painted a uniform "French B" gray so that brightness could not become a factor in the choice. An added precaution was to wash the forms occasionally so as to prevent their becoming soiled and thus characteristically marked;

and finally, when all were learned all were re-painted and presented again.

The regular plan was first to teach the animal to find his food in the apparatus without the forms inserted. This was usually accomplished in one day with a tame animal so that they would cease to beg for food and would explore the apparatus whenever it was brought into the cage. After this preliminary training the forms were inserted and the porcupine allowed to search until he found the food, the experimenter keeping record of all the forms entered or passed without entering. While the animal ate, the whole apparatus was lifted to the other side of the cage, a distance of six or eight feet, the forms were rearranged and the food inserted. Soon after that was done and the experimenter had stepped well out of the way, the animal finished his piece of food, turned around, and approached the apparatus again. This was repeated from 40 to 80, sometimes 150 times a day until the animal's hunger was practically appeased.

The first porcupine to use this apparatus was No. 3 but his reactions were somewhat unsatisfactory both as to method and result. Nevertheless, they are presented in some detail. Four hundred tests were made with the forms six-wise with the food in the circle, and he failed to use the form of the opening as a cue in finding the food, except that he would occasionally refuse to inspect the food receptacle back of the triangle when the apex was down but never when the base was down. There was enough difference in the kinaesthetic experience of entering the two positions to account for all the discrimination present without taking vision into account at all, especially when the animal's behavior was taken into consideration. It was not his custom to refuse to enter the triangle on sight of it; but only at the point of entrance where the kinaesthetic factor would arise. The rectangle was also avoided a few times. This occurred not so much on sight of the form but only when starting to enter would give rise to dermal and kinaesthetic sensations due to its narrowness. These sensations even seemed to be anticipated at times and the inhibition effected before any great advance had been made into the forms. His regular habit was to approach the apparatus at one of the middle forms, inspect the food box behind it, take the next one to the left

and so on until he reached the end and then look around the end. He would then begin with the end one which he had just examined and take them in regular succession until he found the circle, provided he did not turn and make a third and even a fourth exploration of those forms and compartments which he had just examined twice.. As will be seen in the first horizontal column of results (A) in table VI, in no series of 20 experiments each, did he make more than eight correct choices. On the 18th series he made only four correct choices and on the 19th and 20th series five each with an average of 5.7 ± 1.5 correct choices for each of the 20 series.

TABLE VI

FORM STUDY. PORCUPINE NO. 3

Series	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
A	3	3	8	4	5	7	7	8	4	4	8	8	5	7	8	6	6	4	5	5
B	19	15	15	18	18	13	18	18	20	15	16	8	18	9	19	8	19	13	16	10
C	11	14	16	15	14	11	12	17	17	18	17	14	14	12	17	14	15	14	18	16
D	11	11	11	10	10	12	12	9	13	10	12	8	9	10	10	9	11	8	7	11
E	9	11	11	11	13	15	15	13	11	12	8	11	17	19	20	17	19	19	20	18
F	10	12	10	6																

A = Six-wise.
B = Triangle and circle with base down and apex down in alternate series.
C = Circle and triangle with base down.
D = Circle and circle-triangle with curve down.
E = Circle and circle-triangle with apex down.
F = Circle and circle-triangle with curve down.

These results were regarded as negative and the problem made simpler but still the circle was used to mark the food receptacle. The circle was presented pair-wise with the triangle as the latter had stood lowest in the former experiments. By using the triangle, apex down, in one series of 20 experiments and the base down in the next series an attempt was made to determine whether he was observing the triangle as a whole or only some part or aspect of it. The second line of results (B, table VI) shows a steady increase of ability up to the ninth series where there is rapid disintegration of the association when the base of the triangle was down. In the following 20 series (C,

table VI) the base of the triangle was down throughout but he was unable in these 400 tests to perfectly re-establish the association. A compound form was then made consisting of a circle in one-half and of the apex of the triangle in the other half. For convenience this irregular form is called the circle-triangle. This, with its curve edge down, was paired with the circle with the result that in 20 series there were 202 right choices and 198 wrong ones (D, table VI). The porcupine had simply formed the habit of inspecting the one at the left first. This was correct half of the time; and when it was not he would move on to the next one and get the food. The circle-triangle was then adjusted so that the apex was down with the result that it took 12 series to break the place habits. After that the circle was chosen fairly regularly. Only 11 errors were made in the next 160 trials (E, table VI). But when the circle-triangle was again inverted making the base lines of the two forms similar, there was no ability to select the proper one. The animal made only 47% correct choices after feeding in the circle 2000 times previously (F, table VI). At this point, the experimenter became convinced that porcupine No. 3 could not solve the problem assigned to him and the experiment was discontinued. Later experiments about to be described indicate that the failure was due to the method employed, that the experimenter was disheartened too soon and that No. 3 would eventually have mastered the problem. The difficulty in method was to assume a certain proficiency with a given amount of work and promote the animal on the basis of that. This seems to work as disastrously with the porcupine's learning as the same method does occasionally with children in the public school. No. 3's failure to learn the forms was a pedagogical blunder of the experimenter rather than a case of sensory deficiency, or mental incapacity on the part of the porcupine.

Tables VII and VIII representing the results of tests on porcupines Nos. 6 and 7, respectively, require no elaborate interpretation. These animals had recently been captured from the forest, having had, preceding these form tests, only a limited number of tests of righthandedness and lefthandedness. The pair-wise method was employed because this was simpler than presenting all six forms at once. The progress of the two animals with these two forms was almost parallel. Both also

TABLE VII

FORM STUDY. PORCUPINE NO. 6

Cir. & Tr. Apex down	13, 13, 17, 20.
" " " " up	13, 16, 20.
" " " " down	20.
" " C-T " up	12, 12, 11, 10.
" " " " down	18, 20.
" " " " up	15, 13, 15, 15, 15, 10, 13, 9, 11.
" " Sq.	15, 13, 13, 13, 17, 12, 15, 15, 17, 15, 13, 13, 17, 13, 15, 16, 16, 20.
" " El.	20.
" " Hex.	15, 17, 14, 18, 18, 14, 20.
" " H-C H. down	16, 17, 17, 18, 15, 18, 20.
" " " " up	16, 18, 17, 20.
" " Rect.	20.
Six-wise	6, 6, 6, 10, 8, 10, 15, 14, 17. 12, 13, 14, 20.
C, E, C-T, H-C, similar base	16, 20.
" " " " " top	20.
" " " " " left	9, 15, 16, 20.
" " " " " right	14, 16, 20.
Six-wise. New paint	19.

In table VII the initials at the left represent the forms used pair-wise and the positions in which they were exposed. C-T represents the irregular form which corresponds to the circle in one-half and the triangle in the other half. In the same way, H-C represents a similar combination form composed of parts of the hexagon and the circle.

TABLE VIII

FORM STUDY. PORCUPINE NO. 7

Cir. & Tri. Apex down	18, 16, 20.
" " " " up	14, 13, 10, 16, 13, 18, 16, 20.
" " " " down	20.
" " C-T " "	15, 15, 17, 13, 15, 15, 15, 19, 17, 17, 16, 20.
" " " " up	11, 9, 8, 8, 10, 10, 11, 11, 11, 11, 13, 12, 11.
" " Sq.	12, 9, 10, 12, 12, 14, 15, 15, 10, 12, 13, 16, 20, 18, 17, 20.
" " El.	13, 13, 14, 13, 13, 16, 18, 17, 11, 16, 15, 11, 13, 12, 10, 10, 15, 10, 14, 18, 14, 15, 20.
" " Rect.	20.
" " Hex.	17, 17, 20.
" " H-C H. down	20.
" " " " up	20.
Six-wise	12, 11, 16, 20.
C, E, C-T, H-C, similar base	12, 13, 13, 13, 16, 15, 20.
" " " " " top	20.
" " " " " left	15, 20.
" " " " " right	20.
Six-wise. New paint	18.

Table VIII is constructed on the same general plan as table VII.

learned to discriminate the circle from the triangle in both positions as well as from the circle-triangle when the apex of the latter was down; but both failed when the irregular form was inverted, making the base lines similar. The square gave considerable difficulty to both animals. No. 6 suffered three

relapses learning to discriminate between the circle and the square, while the success of No. 7 came suddenly in the 13th series. Fearing that this was a matter of chance, the experiment was continued, but he missed only three in the following 60 tests. The most marked divergence came when the ellipse was paired with the circle. No. 6 required only one series and there was no reason to doubt his ability as he simply looked the apparatus over carefully in his first experience with it, running his nose along the circumference, giving it only a passing glance after that. No. 7 was completely lost with it. It was only after 23 series, with numerous relapses, that he succeeded at all. This difference is not easily accounted for unless it be due to the fact that when No. 6 made his selection his head was raised considerably so that he could see the whole form at once, or at least some of its main dimensions, while No. 7 approached the apparatus with his nose very close to the ground in such a way that when he raised his head to inspect the forms his eye would catch the base lines first. This had been sufficient to enable him to discriminate between the forms before but now other parts of the forms must be closely observed. This explanation is given color by the fact that his success came only after he had been scolded and even punished out of his slovenly habits. At no time did this intrusion of the experimenter occur when the animal was making a selection. He probably succeeded by virtue of a better method which gave him better position and led him to give better attention and not through an increase in ability. Both discriminated the rectangle after they had learned the ellipse. This indicates that only the general features of the ellipse were observed and that if they selected by means of the whole form the margins were not distinctly seen. The hexagon had to be learned but there was never any doubt that they would succeed in avoiding it. It was only a matter of how long it would take them to form the habit well enough to get 100% right choices in any one series. A new irregular form which was half circle and half hexagon was then made. When this was paired with the circle it gave No. 7 no difficulty when either the hexagonal or circular edge was down. No. 6 was never very doubtful about it as the table indicates. The success with the hexagon-circle and the failure with the circle-triangle is no doubt due to practice

and methods of choosing as will be seen in the four-wise tests later.

The six regular forms were then presented six-wise with the circle still marking the food box. The forms were shifted according to a regular schedule which placed the circle in every position an equal number of times as nearly as possible, and successively in a new environment after each reaction. No. 6 had considerable difficulty. His habit of finding the food in the second form approached, came out very strongly. Many times he would reject the first one and plunge into the second without stopping to examine it. In this larger group there was not merely a positive search for the circle but the method employed involved a definite rejection of each of the forms encountered before the circle was reached. The animal would stop before each form separately and "choose" sometimes with considerable hesitation either to enter or not to enter. When the circle was at one end of the apparatus animals have been observed to make as many as 19 "negatives" in succession and finally end by entering the circle first and being credited with a right selection. The animal's behavior in such cases could hardly be misinterpreted. He usually rested his forefeet on the apparatus below the forms, shifted side-wise along the front, frequently looking from side to side as if in search of the circle but never showing indications that he recognized it when he was more than 12 inches from it.

Immediately following these tests the circle, the ellipse, the circle-triangle and the hexagon-circle were presented four-wise (see tables VII and VIII) with the same apparatus used above only removing the end forms and food-boxes. The first arrangement was such that the base lines were all similar while the top halves were parts of the circle, ellipse, triangle, and hexagon. It was not strange that No. 7 with his propensity for using base lines should require three and one-half times as much practice to learn to discriminate the circle from the group. Three of the four errors made by No. 6 were the first three tests, two of which were errors on the circle-triangle. Twenty-five of the 37 errors made by No. 7 were made on the circle-triangle which it will be remembered both animals had previously failed to discriminate when paired with the circle in this position but which both mastered now under more difficult circumstances.

When the forms were arranged so that the top edges were similar, neither had any difficulty. When arranged so that they were similar on the left and at the same time individually symmetrical at top and bottom there was another shake-up in the discriminative ability as the tables indicate. When this was reversed and the forms were arranged similarly at the right, No. 7 was still undisturbed but No. 6 made four errors in the first five tests and then six errors in the next 50 tests. Before discussing those results further it should be added that immediately after these tests the six regular forms were newly painted, and, after three days for No. 7 and 10 days for No. 6, these animals were again tested. No. 7 selected the circle 18 times in the first series and No. 6, 19 times. The "failures" were not so much errors as irregularities.

Four and one-half months later No. 7 was again tested with the apparatus bearing the regular forms six-wise. In the first series he made 11 right choices. The second series only 8 right choices with indications in general behavior, that the association was disintegrating. He was then given the circle and triangle pair-wise. The first series was perfect. The next series with the circle and square was equally good. This was followed by a mixed series in which the triangle, ellipse, hexagon, square and rectangle were paired successively with the circle in the order named, making each form appear four times in the series. Only one error was made and that was what appeared to be an inattentive plunge into the square. In the six-wise tests which followed, the experimenter retired from the room each time while the animal was making his reaction. His results or behavior did not seem to be altered by this absence. His right choices in a series were as follows: 7, 9, 13, 14, 15, 15, 13, 16. This experiment was not carried to completion but it is evident that he was making good progress.

And now it may be well to pause and ask what is the meaning of these results on form discrimination. There is little doubt that the sense of smell has been eliminated, both as a means of locating the food by the direction of the odor and as a means of trailing into the proper form. With No. 3 it is not certain that kinaesthetic sensations were eliminated; rather the indications are that dermal and kinaesthetic factors played the largest part in what selections he made. The behavior of the

other animals, however, does not justify the same assumption. In the cases of Nos. 6 and 7, it is equally certain that there was some form of reaction in which visual stimuli was the chief factor. The two sets of results are sufficiently similar to be discussed as one problem. In reference to other controls, the precautions by means of uniform paints and frequent washings of the soiled forms have most certainly kept the relative brightness of the forms well below the discrimination limen. As to the possibility of the forms having particular markings by which the porcupine might discriminate them, the reader is referred again to the many reversions and inversions of the individual forms which was more than sufficient to complicate and break up any such associations. In the judgment of the writer there remains but two questions legitimately open to discussion: (a) Have the porcupines grasped the forms as a whole or in part sufficient to give a perception of one dimensional space? (b) Have they observed only the outline, only the edge as a more or less regular line giving perception of one dimensional space? Auxiliary to these is the other problem of whether the discrimination is of the perceptual or conceptual order?

As to the first question the author is skeptical. It is far from certain that the animals have even seen a circle or a triangle, as such, in one visual grasp. The fact that the forms were necessarily large for this kind of work; that the animals possess no binocular field of vision and above all that they are probably myopic* in daylight for still objects makes perception

* Note: April 23rd, almost 11 months after I began work with the porcupines and nearly seven months after obtaining porcupine No. 7 this very interesting incident occurred. No. 7 was feeding as usual in one of the side rooms in the barn when a large, light gray cat looked around a partition and stood observing us from a distance of about eight feet. Nothing but its head was visible. As the porcupine turned in the course of the experiment so that the image of the cat might fall on the periphery of his retina, he quilled with a suddenness which I never have seen in any of them before, and scudded away to a corner of the room. When I looked again the cat was still motionless but whether it had moved its head as the porcupine turned in the experiment and thus gave No. 7 vision of a moving object or whether the porcupine had perceived a still object at that distance can not be certain. This difference in the behavior of the porcupine toward food stimulus and enemies is significant. I have been told on reputable authority that a few months after I had captured this animal, hunters took a wild cat from the same locality in the mountain. Thus, we may safely surmise that No. 7 phyletically and probably individually knew this feline enemy. Possibly if we were stimulating the porcupine with their arch enemies we should not be led to infer myopia. But the fact still remains that the porcupine seems to be unable to perceive significance in a situation as new as a piece of laboratory apparatus at a distance of more than a few inches.

of the whole form physiologically improbable if not impossible. Add to this the fact that the forms were empty forms without a constant background and the case is more evident.

The second question for discussion is answered in part by the first for the two are, in a large measure, alternative. The suspicion that the porcupines might be observing only the boundary line led to the use of the irregular forms in the pair-wise study. Suspicions become almost, if not wholly, convictions during the tests with the irregular forms four-wise wherein discrimination must be a part of the form only. In choosing these forms the porcupine would run his line of regard, or visual axis, around the form till he came to the part which differentiated it from the others. He would often give a quick start as of surprise when he reached such a part, hold the eye and face steady for a moment as if to reassure vision, sometimes stretch the neck and body toward the point of interest and then pass on. When he reached the circle he would again trace the edge of the form with the eye and as he came to the starting place, usually the base, without any interruption, he would enter and secure the food.

The kinaesthetic elements which enter in this explanation will be classed as secondary visual factors and not as separate criteria in themselves. This conclusion was not what was expected when the apparatus was designed and the experiment begun. The animal's behavior before the regular forms, five of which were symmetrical in regard to the central point was not sufficient to bring out the phenomenon. It was more than that of throwing the line of regard from side to side in rapid, irregular movements without any of the definite exploitation which occurred with the irregular forms.

It remains to be determined whether the reaction is anything above a simple percept of the whole, a part, or a quality, accompanied by a memory of a sufficiently high character to bring about a positive rather than a negative reaction toward it. For instance, when the animal refused to enter the wrong forms 15 to 20 times and ended by reacting definitely and positively to the circle the first time he encountered it in the search can it be said that he knew what he was searching for while he was searching, or that he went about it with an indefinite image of the thing toward which he reacted positively when he

encountered it? In other words, shall it be assumed that the animal had no mental grasp of the situation except when that situation was definitely intruding itself upon the senses? Is there anything more than "present knowledge" as Judd uses that term? It would be presumptuous, of course, to answer this much debated question of the existence or non-existence of free ideas in the animal mind. No better opportunity will be found for discussing it, however, than in connection with form study. Morgan (29) grants that animals habitually form mental constructs by immediate association analogous to, if not resembling those in the human mind; and also that animals are accustomed to define these constructs definitely by examination. He also holds that "a sensation or a group of sensations may suggest a series of reconstructed or a series of remembered phenomena." If this were only demonstrated, one would feel safe in saying that the porcupines possessed memory images of the circle while they were looking for it. The same author doubts the power of an animal to form "isolates" but admits that they may have, for instance, "vague representations of things good to eat in which the character of eatability is predominant." Thorndike found no indication of ideas in his cats, dogs and monkeys but Cole (10) gives results which seem to imply ideas in the raccoon. Miss Washburn (40) leaves the matter of "memory ideas" where she found it, lamenting the "lack of more definite knowledge on the subject," (p. 273). This problem implies more than is involved in the question of the mental content of the porcupine while he is seeking the circle. On the other hand, the phenomenon may be more analogous to the state in which most people would find themselves if they were required to react to a certain bell, sounded promiscuously among other bells. They would probably not hold an image of the tone while waiting for it, but would respond no less readily when that particular tone struck the ear. One does not need to infer free ideas to explain this phenomenon of "rejecting" and "choosing" the forms. This would seem to be the most plausible interpretation of the mental content of the porcupines during their reactions to the forms.

Discrimination of Color and Brightness.—In testing the ability of the porcupine to discriminate colors and brightnesses as a means of finding the proper food-boxes, the standard blue and

green colored papers of the Bradley series, and Nos. 1, 11, 21, 32, 41, 45, and 50 of Nendel's gray papers have been used as stimuli. This heterogeneous light was made necessary by the fact that the apparatus for obtaining monochromatic light was not available. Had the means for obtaining pure color been at hand such as was recently described by Yerkes and Watson (52) the problem would have been very much more definitized. But just as the apparatus is more highly specialized, the problem and the results are equally limited and specialized. If pure color is used as a stimulus, the conclusion must be that the animal can or cannot, rather does or does not use pure color in determining his reactions. With more definite apparatus results would, no doubt, have been more highly satisfactory, especially in the quantitative brightness reactions. However, it is not entirely safe to draw conclusions concerning an animal's ability to tell color in general when it has been tested only in a laboratory dark room with a form of stimulus which it had never encountered either individually or in its phyletic development. The form of the stimulus used carries out the general policy of this study: viz., to keep all conditions as near to the natural environment as possible. The method employed at least outlines the problem in a broad and general way and at the same time reveals certain unique features of the learning process.

Similar precautions in regard to odor of food, trailing and special markings were exercised in designing the apparatus and carrying out this series of experiments as were observed in the form tests just reported. The food boxes were made of one-half inch lumber four and one-half inches square inside measure and four inches high. They were made first from four pieces of lumber forming a hollow cylinder from which the boxes were sawed with great care to get them of the same height. These boxes had no bottoms but were set loosely on a base-board raised five inches from the ground. Thus, the food never touched the box, but left most of its residual odor on the base-board which remained constant throughout the experiment. In addition, a secret pocket was made on the base-board inside the box in which a piece of the kind of food being used was concealed so that either box which the animal might approach contained food; but he soon learned that he would not be

permitted to tear off the pocket and get the food which was in it. He must find the box wherein there was food which was obtainable. In the search for food, color or brightness was introduced as guides. The boxes were covered on the outside with colored or gray paper. A two-inch strip of the same paper was placed vertically in the middle of each of the inner sides. It will be observed that each box had eight different positions in which it might be presented to the animal. It could be turned so that the animal must approach one and then the other of the four similar sides and reach across each of the four edges in turn. Then the box could be inverted to present an equal number of variations in the second position. Special precaution was taken not only to shift the position of the boxes according to a regular schedule but also to rotate and invert them in such a manner as to break up any secondary association which might be formed. Porcupines Nos. 3 and 10 were used in this experiment and the results are shown in tables IX and X respectively.

TABLE IX

COLOR AND BRIGHTNESS DISCRIMINATION. PORCUPINE NO. 3

Blue-Green.....	10, 11, 11, 13, 16, 11, 7, 12, 11, 14, 10, 14, 12, 10 (failure).
Grays 1–50.....	11, 10, 13, 12, 14, 10, 12, 12, 13, 10, 14, 13, 11, 15, 14, 13, 16, 17, 18, 20.
" 11–50.....	20.
" 21–50.....	20.
" 32–50.....	20.
" 41–50.....	20.
" 45–50.....	13, 13, 15, 13, 14, 15, 13, 9, 13, 13, 15, 17, 14, 15, 15, 12, 12.
" 41–50.....	12, 11, 13.
" 1–50.....	20.
" 1–45.....	20.
" 1–41.....	20.
" 1–32.....	20.
" 1–21.....	20.
" 1–11.....	14, 20.
" 1–11 (New)..	12, 14, 17, 20.
" 11–41.....	20.
" 21–32.....	14, 18, 14, 12, 12 (failure).
Blue-Green.....	18, 20.
B^2-G_2 Alt. B_2-G^2...	18.
B_2-G^2.....	18.
B^2-G_2 Food in light	10.
Blue-Green.....	13, 19.
B^2-G_2 Alt. B_2-G^2...	18.
B^2-G_2 Food in light	11.
B^2-G_2 Alt. B_2-G^2...	17.
B^2-G_2 Food in light	6.

It should be noted that No. 3 had been undergoing daily experimentation for four months while No. 10 had been out of

the woods only a few weeks and had had no experience except in a few preliminary hand reactions in connection with the taming process. No. 3 was also mature while No. 10 was not half grown. It is not certain how much either of these factors affected the results but one would expect that the experience and steadiness of No. 3, contrasted with the constitutional wildness and inexperience of No. 10 would be sufficient to account for some of the difference in the two tables or records. Stimuli were used pair-wise for the reason that the porcupine's peculiar manner of search as explained in the form tests make six-wise stimuli confusing, if they are presented as new problems.

TABLE X

COLOR AND BRIGHTNESS REACTIONS. PORCUPINE NO. 10

Blue-Green.....	15, 8, 11, 7, 9, 12, 16, 8, 15, 7, 12, 12, 9, 9, 13, 9, 11, 14, 11, 10 (failure).
Grays 1–50.....	11, 11, 11, 11, 13, 12, 12, 14, 14, 16, 14, 14, 20, 20.
" 11–50.....	17, 17, 16, 18, 14, 14, 17, 20.
" 21–50.....	16, 16, 18, 18, 18, 17, 15, 14, 14, 18.
" 32–50.....	15, 17, 17, 20.
" 41–50.....	15, 13, 14, 20.
" 41–50 (New)..	10, 13, 15, 15, 15, 14, 15, 14, 15, 13, 12, 15, 15 (failure).
" 32–50.....	16, 17, 18, 20.
" 41–50.....	18, 12, 20.
" 45–50.....	13, 10, 10, 12, 15, 16, 15, 12, 15, 20.
" 45–50 (New)..	13, 14, 16, 15, 12, 12, 16, 15, 16, 10, 10, 8, 9 (failure).
" 41–50.....	18, 20.
" 45–50.....	11, 15, 17, 12, 13 (failure).
" 1–50.....	20.
" 1–41.....	20.
" 1–32.....	20.
" 1–21.....	17, 20.
" 1–11.....	14, 15, 15, 17, 16, 20.
" 1–11 (New)..	18, 20.
" 11–41.....	15, 20.
" 21–32.....	10, 10, 14, 16, 15, 15, 16, 14, 10, 15, 12, 12 (failure).
Blue-Green.....	17, 16, 14, 16, 17, 20.
B^2-G_2 Alt. B_2-G^2...	16, 15, 18, 18.
B_2-G^2.....	18.
B^2-G_2 Food in light	9, 11, 9.
Blue-Green.....	12, 17.
B^2-G_2 Alt. B_2-G^2...	17.
B^2-G_2 Food in light	7.

In tables IX and X, except where otherwise stated, food was placed in the darker box. B^2-G_2 represents light blue and dark green respectively. In like manner, B_2-G^2 represents dark blue and light green. The figures to the right indicate the number of correct choices in series of 20 each. B^2-G_2 Alt. B_2-G^2 indicates that in those cases the pairs were exchanged after each test.

The method employed was very similar to that of the form tests, excepting that the experimenter remained seated through-

out the feeding period lifting the apparatus from right to left and vice versa after each test. After the animal had eaten his food, he turned, passed in front of the experimenter and about three feet beyond, with his face turned from the experimenter, while the selection was being made. The space between the successive positions of the apparatus was sufficient for a new adjustment of the animal's movements before he made the next choice and the distance from the experimenter was greater than that at which the porcupine was ever known to react from a visual stimulus of a still object.

The experiment was begun by presenting the blue and the green boxes, with food in the blue. No. 3 was given 14 series of 20 each and No. 10, 20 series. The results were negative. Neither porcupine appeared to be making progress and No. 3 renewed his habit of going to a particular position as he had previously done in the form tests. Experiments with colors were temporarily discontinued and white and black boxes (Nendel's grays 1–50) were substituted. The standard of excellence arbitrarily determined was 100% correct choices in any one series of 20 tests. On this basis No. 10 learned to choose the black in 13 series while No. 3 required 13 series to break up his place habit. After that there was improvement and he made the 20th series perfect. Light gray was then substituted for the white, thus varying the comparison box and leaving the food box constant. No. 10 was very much confused but No. 3 was not confused in the least. When gray 21 was inserted in place of gray 11, porcupine No. 10 was even more confused than before, but No. 3 was again not disturbed. When grays 32 and 41 were successively introduced No. 10 was thrown off each time but No. 3 was still master of the conditions. Up to this point the conditions of the experiment were identical for the two animals. It was thought best, however, to modify the method for the different animals. No. 3 was given gray 45 as a comparison box but failed in 17 series. Table IX shows that he was no nearer the mastery at the end than he was at the beginning and was so confused that when he was given gray 41 again as a comparison he failed with that. Porcupine No. 10 was given new boxes with grays 41 and 50 but was not certain of them after 13 series. In seeking an explanation for this it was discovered that Bradley black had previously been used instead

of a Nendel black. As the former had a smooth and the latter a velvet finish, the difference in brightness-tone may have been sufficient to confuse the animal, especially as the difference between the stimuli was near the discrimination limen. When gray 32 was inserted giving eighteen instead of nine shades of difference he soon regained his ability. Later he succeeded with a difference of nine shades and even made the 10th series perfect when gray 45 was paired with gray 50 but this habit disintegrated when he was given newly papered boxes. Gray 41 was again restored as comparison and he had little difficulty; but he was lost when another attempt was made to test his ability to discriminate between two grays differing by five shades. Additional tests were then made toward the other end of the scale of grays by varying the food-box stimulus and leaving the comparison box constant, always feeding in the darker box. The steps down to gray 11 gave no disturbance worthy of mention. With these grays, however, both animals had difficulty. Likewise, both were somewhat confused when new boxes were presented after they had succeeded with the first pair. It is significant, however, that No. 3 made 23 errors with six series in learning to distinguish grays 1–11 while No. 10 made 25 errors with eight series. The difference is sufficiently slight to bring them into the same category for the purposes of discussion. It is evident that this is near the limen of the porcupine's ability to discriminate shades of brightness when presented as shaded papers,—a limen very much higher than that reported by Cole (10) for the raccoons. Following this, the shade of both the food box and the comparison box were varied toward the middle grays with food still in the darker box. Neither animal had any great difficulty with grays 11 and 41 but both failed completely when grays 21 and 32 were paired. This, at first, seems strange since there was a difference of eleven shades between those two grays and the porcupines had learned to distinguish a difference of nine shades at the dark end and of ten shades at the light end. But even to the human eye discrimination is easier where black or white are presented as one factor in comparison. It is not strange that the same thing is found in the reactions of the porcupine.

It is offered as a suggestion rather than a conclusion that in discriminating the black and white boxes at first, No. 3 made

his selection by choosing the black while No. 10 learned to select by avoiding the white—the one reacting positively and the other negatively. The reason for this is evident in the records. When the food box remained constant and the comparison box varied, No. 3 was not confused in the least while No. 10 had to relearn the task after every change. In the last part where the white remained constant No. 10 had very little difficulty with the changes for the first time in his experiences. No. 3 also had little trouble in the last half and that is all that is lacking to make it a thoroughly good hypothesis, though his experience may account for the improved results in the last part of the experiment. Yerkes (49) reports a similar experience with a mouse which had to relearn grays when they were changed though the relative brightness was not greatly altered. The results in each case illustrate the very strong influence of every condition of the environment as well as the concreteness of the reactions of the animals and the inability of the animal to generalize even in the most obvious cases.

Discrimination of Color following Experiments with Brightness.—After completing the brightness tests reported above the experiments with blue and green stimuli were repeated. The same animals were used as subjects. Both had failed in the earlier series but after the experience with the brightness in which they had formed a habit of going to the darker box and with food in the blue box, one chose blue 17 times and the other 18 times on the first series. While No. 3 was much more certain of his choice, there was never any doubt about No. 10's success. Following this, control experiments were made to determine if possible whether the animal was choosing the blue box because it was blue or because it was the darker of the two.

A pair of boxes covered with blue "shade 2" and green "tint 2" of the Bradley colors, also a pair with blue "tint 2" and green "shade 2" were used. The first pair was a very dark blue and a very light green while the second pair was the reverse, very dark green and very light blue. These pairs were presented to the animal in alternate individual experiments, feeding in the dark blue with one pair and in the dark green with the other pair. Colors in each experiment were alternated and at the same time the darker box continued to be the food box. No. 10 made 84% correct choices in four series and No.

3 made 18 correct choices in the first series. In this, there is a strong indication that brightness was the cue in the colored boxes rather than the color itself. The porcupines did not behave as if they were in the presence of a new problem but rather in the presence of a slight variation of a familiar condition. Each of them was given a series with the dark blue and the light green and both chose the blue box 18 times. Following this a series was given with the light blue and the dark green but this time the food was in the lighter box. It was assumed that if they were reacting to color they would still have blue as a stimulus but if they were reacting to brightness they would have a new problem. No. 10 made less than 50% in three series and No. 3 got just what chance would give him. This was somewhat surprising for if they were not reacting to color they would not be expected to do so well as that. Each of the animals, however, went to the dark box first and when they failed to find food, looked toward the light one but refused to inspect it and examine it for food. Instead, they each turned around and begged from the experimenter. Failing in that, they returned to the dark box again. No. 3 then looked into the light blue box and obtained food but No. 10 appealed to the experimenter again and again before he explored the light blue. This was exactly the habits of the animals during earlier tests with grays and colors on the few occasions when food was not inserted or when it was placed into the wrong box by the experimenter. This leads the writer to believe that they had used the same basis of choice which they had formerly used in the gray tests. This last succession of tests was repeated with both animals with the same general results as shown in the tables, a third repetition with No. 3 brought even more decided results.

TABLE XI

PORCUPINE NO. 3. (MEMORY 105 DAYS)

Black-White	15, 17, 20.
Blue-Green food in blue	12, 15, 16, 14, 13.
Black-White	20.
Blue-Green food in blue	17, 17, 15, 20.
B^2-G_2 Alt. G^2-B_2 (dark)	17, 17, 19, 16, 17, 18, 20.
B_2-G^2 food in dark	16, 18, 18, 20.
B^2-G_2 food in light	7, 9, 9, 5, 5.
B^2-G^2 food in blue	10, 13, 12, 17, 14, 12, 10.
B_2-G^2 food in dark	20.
B_2-G_2 food in dark blue	14, 16, 20.
Blue-Green food in blue	16, 18, 20.

TABLE XII

PORCUPINE No. 10. (MEMORY 105 DAYS)

Black-White	Records accidentally destroyed.
Blue-Green food in blue	15, 12, 17, 18, 17, 17, 17, 14, 13.
Black-White	20.
Blue-Green food in blue	17, 16, 17, 17, 20.
B^2-G_2 food in light green	14, 12, 7, 12, 5, 10, 10.
B^2-G^2 food in blue	15, 11, 10, 10, 8.
B_2-G^2 food in dark blue	10, 7, 13, 16, 13, 20.
B_2-G_2 food in dark blue	17, 14, 11, 11, 12, 13, 12, 20.
Blue-Green food in blue	17, 16, 20.

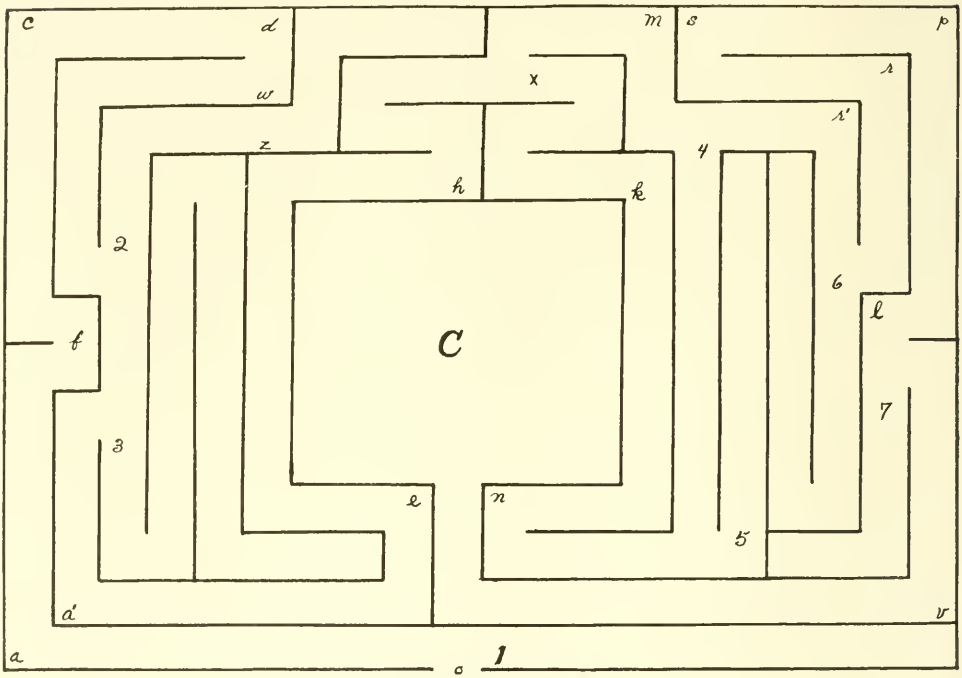
Tables XI and XII show the memory of these two porcupines on the color and brightness reactions and also add another check on the problem of whether these animals were discriminating by color or brightness in their selection of the food box. Let it be remembered that the index figure indicates the relative brightness of the colors used, i.e., B^2 G^2 representing the light blue and the light green respectively while B_2 and G_2 stand for the darker shades of the blue and green. The black-white records for No. 10 were unfortunately lost but they differed in no essential feature from those of No. 3. Again the two animals give practically the same results. Both fail or at least do not progress with the blue-green stimulus after relearning the black-white, but both make 100% in the first series when black-white is restored and move steadily to success with the color boxes afterwards. No. 3 was tested on the original alternating series in which the "food" color was changed from blue to green and vice versa with each experiment, the food always being in the darker box. He made 16 errors in seven series, whereas with chance he should have made 70 wrong choices. In the experiments which follow the alternating series with No. 3, a regular plan was followed in which blue was maintained as the "food" stimulus while its relative brightness was varied from time to time till brightness had been reversed. Both failed when brightness was nearly equal and both changed color to feed in the darker box when brightness was restored. Both later succeeded with blue-green as they had previously; but both just as completely failed when food was placed in the relatively lighter box and there was no better success in solving the problem when both the light tinted boxes were paired. This would appear to be a color difference sufficiently characteristic if they were reacting to color, whereas the bright-

ness difference is probably less than 10 shades in the Nendel's gray series which have been shown to be near the discrimination limen for these two animals. One would expect them to get 50% correct choices by chance alone. In reality No. 3 got 62% and No. 10 only 54% correct. Their behavior was even more convincing of the fact that they were not making any discrimination between the boxes. When No. 3 was given dark instead of light blue with the light green he was able to discriminate at once. No. 10 required some time to break his habit of always going to the right hand side. When the dark green was put in with the dark blue there was another confusion resulting rather seriously for No. 10 but from which he recovered. Explanation should be made of why the experimenters did not conclude that No. 10 had failed with the darker shades when for five series he made only a little more than chance would give him. The explanation lies in his general behavior which the table cannot show and in the fact that he had 80.5% correct choices in the first series. Even with that long struggle there was never a doubt about the final outcome. In the case where the light tints were used each of the animals convinced the experimenters in five series that they could not, or at least would not extricate themselves from the difficulty. This is not a personal factor of the experimenter but is an observation arising from long experience with the porcupines.

In final comment on color and brightness reactions it can only be said that the results cannot warrant the conclusion that the porcupine can discriminate and react to colors in an intelligent manner. They can distinguish boxes which are blue to the human eye from boxes which are green to the human eye, both in the standard Bradley colors and in the darker shades; but it is most probable that the difference in brightness in these two colors is also above their threshold of brightness discrimination. No satisfactory means has been devised for determining by physical methods just how much grayness there is in each of these colors. Various photometric determinations have been made in twilight, contrast rings, comparisons by flicker photometry, etc., but no trustworthy results have been obtained. Furthermore, as the grays which appear to correspond with the colors used vary with each change in intensity of light, and as the experiments with the animals were

performed under natural conditions of illumination one is not justified in specifying what brightness difference there was between the two colors during the experimentation. More exact apparatus and more constant conditions are needed. The psychological aspect of the problem must wait its final solution on the mastery of the more physical, physiological and biological problems which are auxiliary to it. In the mean time it is safe to say that so far as heterchromatic stimulation is concerned (and that is the only kind the porcupine has ever had to develop its sense organs) the porcupine discriminates a brightness difference of about 10 shades of the Nendel series of grays. On the other hand, it cannot be said that the eye of the animal is not sensitive to color stimulation even though no evidence of color discrimination was obtained. But one thing is sure: viz., they have not reacted to color differences in determining their behavior as readily as they have to brightness differences. This is as close as Miss Washburn, with her more delicate apparatus, comes to a solution in her recent study of the rabbit (41). Such might well be the situation with a nocturnal animal even though their eyes should mechanically analyze chromatic light. The phyletic history is not known as to how long and to what extent the porcupine has avoided the daylight and consequently it is not certain how many engraphings of earlier experience are present but not now functioning. The relative pre-perception, so to speak, for brightness and color stimulation might also be inquired into. Is it probable that *sensibility* is always equivalent to *sensability*, if one may adapt a word to the occasion? In other words, how much is an animal's activity dominated by sensory stimuli as such, and how much by the phyletic and individual significance of the stimuli which are effective? These problems are as yet scarcely beyond the *a priori* stage. It is doubtful if any experimental methods now designed will throw much light on the subject in its broader aspects. However, a consideration of some of them may be found helpful in throwing light on laboratory results. It may be found necessary for the laboratory experimenter to invite the geologist as he has the naturalist and biologist to aid him in interpreting his findings. The vagueness of results in this and most other studies on color vision seems to indicate that there are important factors not yet recognized in the problem of color vision in animals.

Threading Maze.—For maze or labyrinth experiments, the same apparatus of the Hampton Court type was used which Kinnaman (26) had formerly used with the monkeys. His description is as follows: "My maze was 17 feet long, 13 feet wide and 14 inches high. The alleys were one foot wide. The whole was built of chicken wire fastened to wooden frames. The central portion was not covered * * *. The entrance was at o. Numbers 1–7 indicate the blind alleys. The dotted



The Hampton Court Maze

line shows the most direct course through the maze. By the shortest path it was 105 feet from the entrance to the food." In the accompanying cut the numbers and letters correspond with the above description. Most of them are advantageously placed for discussing the behavior of an animal in the maze.

The method of counting errors was similar to the methods of most other experimenters with mazes, though not in accordance with the recommendations of Miss Hicks (22) except as indicated below. If the animal entered one of the seven blind

alleys even to the end and returned directly he was charged up with one error. If he ran back and forth while inside, each false turn was counted an error of equal value as was also his failure to take the proper course on coming out. To turn back when on the right route provided he made any progress was scored an error—mere turning around not being so considered. Entrance into *x* was called an error if he returned and took the short course but not if he eventually took the long course. Entrance at 4 was considered an error as he could never make progress if he formed the habit of taking that path. The experimenter found it most convenient to take a position in the center while making records. This seemed most advisable as it took away the strong counter stimulus of the animal trying to get to the experimenter for food if he were in sight in another direction. In addition, it gave the normal stimulus that where the experimenter was there was the food, and thus kept the animal at work. On the other hand, the maze was so constructed that a strong stimulus in the direction of the center tends to cause the animal to enter each of the cul-de-sacs, unless it should be 5. The correct route leads away from the center at the parting of the ways in other cases. Thus it would seem, that the presence of the experimenter in the center, increasing the stimulus many fold it may be, would interfere with the progress of the animal rather than assist him in finding the way.

The results obtained with No. 6 lend themselves very well to a somewhat detailed study. This porcupine had just finished the form tests and was thus completely tame though he had the following tendency less strongly than some of the other animals included in this study. He had always to be coaxed out of the den for experimentation. When he was once out, however, he worked steadily to the close of each series of experiments.

The maze was located in the basement of the barn in a room with a floor of earth. The room was well lighted by two large windows on the side opposite the entrance of the maze. The living room of the animal was in the direction marked *p* on the chart at a distance of 12 feet from the nearest edge of the maze.

Experiments were performed daily for one week as follows: First day, 1; second day, 2; third day, 5; fourth day, 12; fifth day, 12; sixth day, 16. Details in time and errors for this series of experiments are presented in table XIII.

TABLE XIII

PORCUPINE NO. 6 IN MAZE

No.	Time	Error	Error Revised	Comb.
1	1 hr. 9 min. 4 sec.	76	85	84.3
2	1 " 15 " 40 "	63	77	83.9
3	1 " 24 " 20 "	93	105	103.1
4	28 " 10 "	15	17	25.4
5	26 " 20 "	19	19	25.1
6	12 " 50 "	14	14	14.9
7	10 " 45 "	10	15	13.9
8	21 " 30 "	26	32	28.9
9	8 " 10 "	7	7	8.4
10	5 " 10 "	7	7	6.6
11	5 " 0 "	8	8	7.0
12	4 " 50 "	9	9	7.4
13	4 " 20 "	7	9	7.1
14	4 " 20 "	8	10	7.8
15	2 " 45 "	3	3	3.1
16	3 " 30 "	7	9	6.6
17	2 " 15 "	4	5	3.8
18	2 " 58 "	7	9	6.2
19	3 " 2 "	5	6	4.8
20	5 " 12 "	2	2	2.7
21	4 " 38 "	4	5	5.2
22	2 " 23 "	3	5	3.9
23	3 " 17 "	5	6	4.9
24	1 " 50 "	1	1	1.6
25	2 " 10 "	2	3	2.8
26	2 " 38 "	4	5	4.0
27	1 " 49 "	2	2	2.0
28	1 " 35 "	2	2	1.9
29	2 " 18 "	3	3	2.9
30	2 " 5 "	0	0	1.2
31	2 " 2 "	2	3	2.1
32	2 " 32 "	4	5	4.0
33	8 " 38 "	8	11	8.9
34	3 " 12 "	4	5	4.4
35	3 " 46 "	8	11	7.7
36	1 " 55 "	2	2	2.1
37	1 " 28 "	2	2	1.8
38	1 " 57 "	3	3	2.6
39	1 " 13 "	0	0	.4
40	1 " 4 "	0	0	.3
41	1 " 6 "	0	0	.3
42	1 " 6 "	0	0	.3
43	1 " 6 "	0	0	.3
44	1 " 10 "	0	0	.4
45	1 " 12 "	0	0	.4
46	1 " 7 "	0	0	.3
47	1 " 10 "	0	0	.4
48	1 " 10 "	0	0	.4

First day. Experiment one. The porcupine consumed almost an hour and ten minutes and was charged up with 76 errors in making the trip. Ten minutes were consumed in getting started. The animal only reached the point marked *b* in that time. Thirty-

four minutes were spent near the center *e-h*. Seventeen minutes were lost between *r* and *6*. The remainder of the time was spent at various places in efforts apparently directed to getting out of the trap in which he found himself.

Second day. Experiments 2 and 3. Time was greater on the second effort by six minutes than the previous time but there were 13 fewer errors. Greatest difficulty encountered was in *3*. Although *e-h* caused little difficulty there was even greater confusion between *6* and *r* than occurred on the former test. Certain parts seemed to induce hesitation and confusion. The second trial on this day consumed nearly an hour and 25 minutes, an increase of nearly nine minutes with an increase of 30 in the number of errors. There were, however, two outside distractions which caused serious difficulties by inducing the animal to retrace his path.

Third day. Experiments 4 to 8 inclusive. The five trials on this day were made possible by the fact that the animal materially reduced his time. There was nothing else especially characteristic of the day. On the last trip, he did the last half of the maze in 45 seconds but even then the time was increased from 10 minutes and 45 seconds to 21 minutes and 30 seconds, due to confusion in the first half.

Fourth day. Experiments 9 to 20 inclusive. The porcupine found blind alley *2* and entered it on each of the 12 trips on the fourth day. Next to the discovery of *2* the most important feature was his confusion at *r'* and *r* on each of the first six for the day with the subsequent solution so that the last half of the maze was traversed without error. This confusion was interesting from the fact that the two points were similar but required opposite reactions to pass them, confusion probably being due to the mistaken identity of the two positions. The average time for the 12 excursions was 4 minutes and 11 seconds. The time for each of the first six was above the average and that for the last six was below the average. It is also significant that the curve of the day is low at the middle with a minimum of 2 minutes and 15 seconds on the ninth trip. The increase in time on the last excursions was probably due to fatigue and partial satiety.

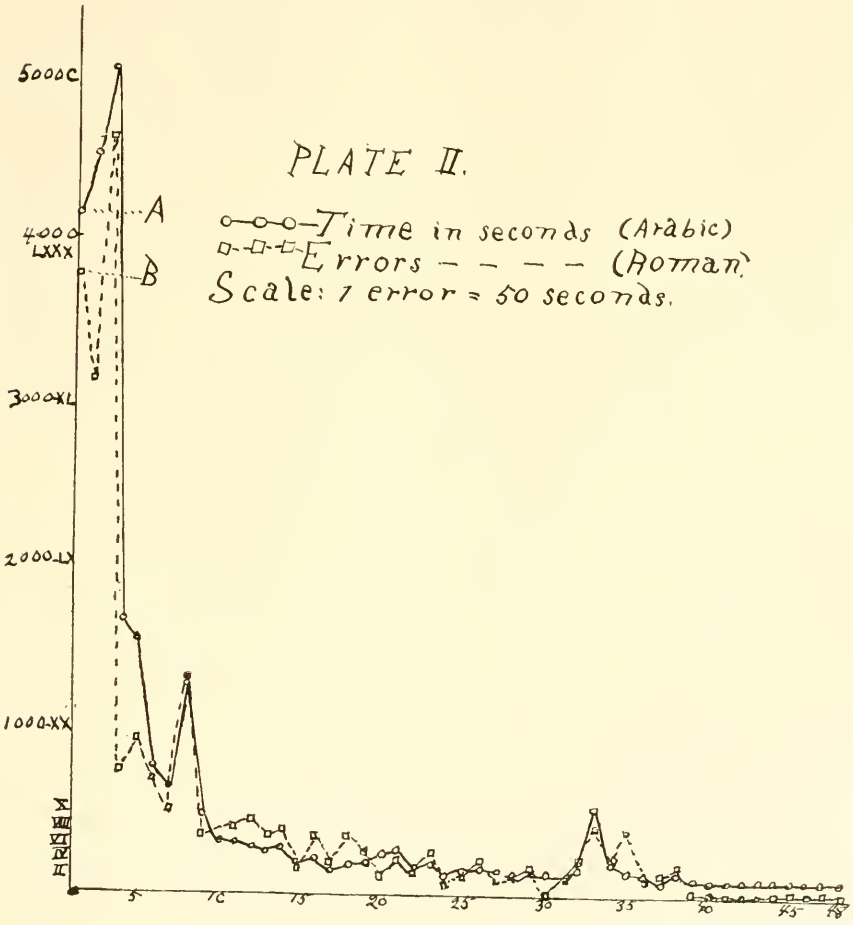
Fifth day. Experiments 21 to 32 inclusive. Twelve experiments were also made on this day. The 10th for the day and

30th for the series resulted in the first errorless trip. That the time was not also minimum deserves a word, as there was some hesitation at *p* but no turning back. The porcupine made the start perfectly on the fourth, seventh, eighth, and tenth trials. He missed blind alley *2* on all except the sixth, ninth, eleventh and twelfth. This showed marked improvement over the fourth day's work. There was but one error made in the second half on the fifth day, that was on the first excursion when the former confusion of *r'* and *r* recurred. The average time for the day was 2 minutes and 26 seconds, almost one-half the fourth day's average with only four excursions requiring more than this average and with another drop near the middle of the day's curve. This may be thought to indicate that work was being continued too long but the next day's experience does not entirely justify such a conclusion. The average time for traversing the last half of the maze was 47 seconds with only two above this average—the first trip requiring one minute and the tenth requiring 1 minute 15 seconds.

Sixth day. Experiments 33 to 48 inclusive. The animal was allowed to go through the maze 16 times on this sixth day. On the seventh trip for the day or the 39th for the series, he again traversed the maze without an error, and in 10 successive trials he made errorless trips. This had arbitrarily been classed as the standard of perfection. It is the same standard which Kinnaman used and so might furnish a basis for comparison where comparisons are desirable.

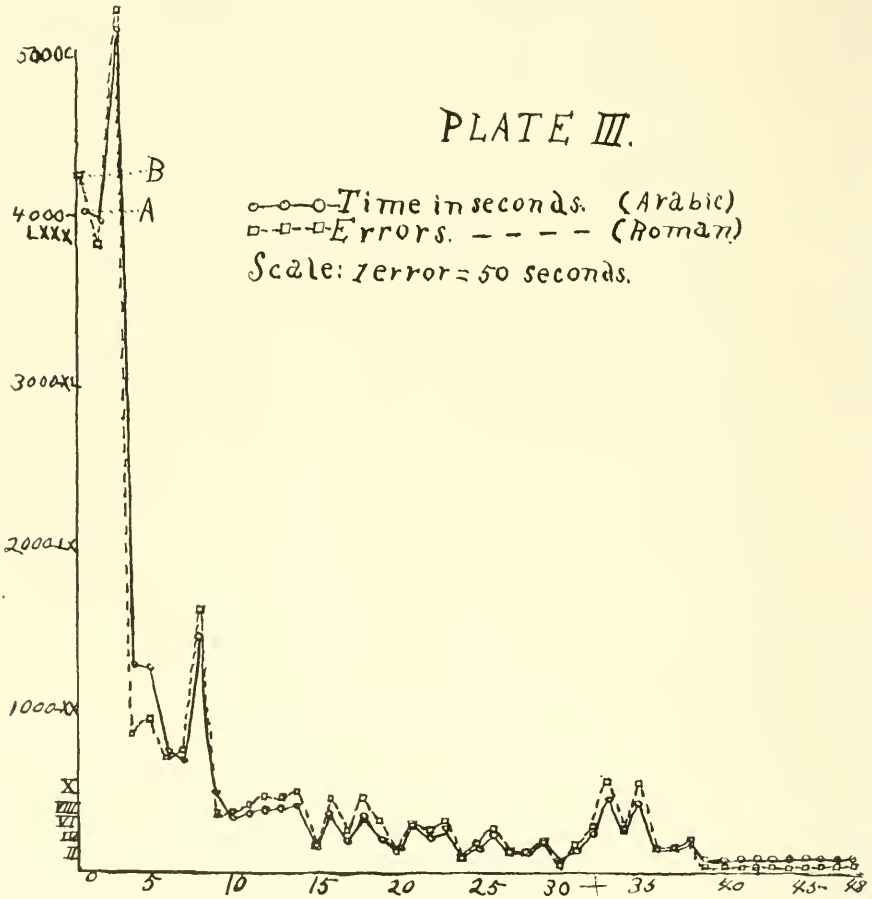
A glance at the curves in plate II, which are based upon figures in the first two columns of table XIII, shows that the learning curve of porcupine No. 6 with the maze is not unlike the curve of No. 11 with the puzzle-box (plate I) and that both conform closely to the curves of other animals in these and similar tests. It is also noteworthy that the time and error curves are generally parallel from which it may be inferred that either variable is a fair measure of the animal's progress in overcoming difficulties of this kind. This latter observation is all the more important in view of the attack upon the single variable method which Miss Hicks (22) has recently made. To satisfy this new issue which has been raised, errors have been recalculated so as to include not only the criteria mentioned above, but also each straight small segment of the maze as a

unit of error on the returns. These results have been separately tabulated and have also been combined with the time data as Miss Hicks recommends. These results are shown in the last two columns of figures in table XIII. Repeated efforts have been made to superpose these latter curves upon the time and



error curves in plate II, but it has been found impracticable. Consequently, a curve based upon Miss Hicks' recommendations for counting errors (curve M) and also a curve which combines the revised error and time data (curve N) are shown in plate III. The writer is unable to determine wherein the

curves of plate III tell a truer, more complete or even a materially different story from those of plate II. They are submitted for scientific completeness and in the hope that others may find more value in them than this study has been able to appreciate. The writer's own opinion is that the method of counting



errors by segments of the retraced path is faulty in that retracing through 10 segments in succession does not represent 10 successive errors but rather one error with its logical consequences, chief of which are motor factors, among which may

be a minimum of mentality,—the thing the data assumes to measure.

Kinnaman's (26) male monkey using this same apparatus made his first errorless trip on the 36th trial, and did not make 10 errorless trips in succession until the 113th. Porcupine No. 6 (male) reached his first success on the 30th trip and perfection in the 48th. Porter's (32) sparrow in a maze of this pattern made the first errorless trip on the 23rd but never attained the perfection of 10 successive errorless trips. Kinnaman's female monkey attained the first success on the 13th and perfection on the 66th. The female monkey was thus earlier than No. 6 in reaching the first success but not so ready in fixing the habit as was the porcupine. The nearest approach to the first success of the female monkey which has been observed was with porcupine No. 4, which made only one error on the 13th trip and went without error on the 19th and 27th trials. The error on the 13th was a very short excursion down into blind alley 1, a negligible error but for scientific accuracy. No. 6 was surpassed by the female monkey and the sparrow on first success but he surpassed them all in gaining the arbitrary standard of perfection.

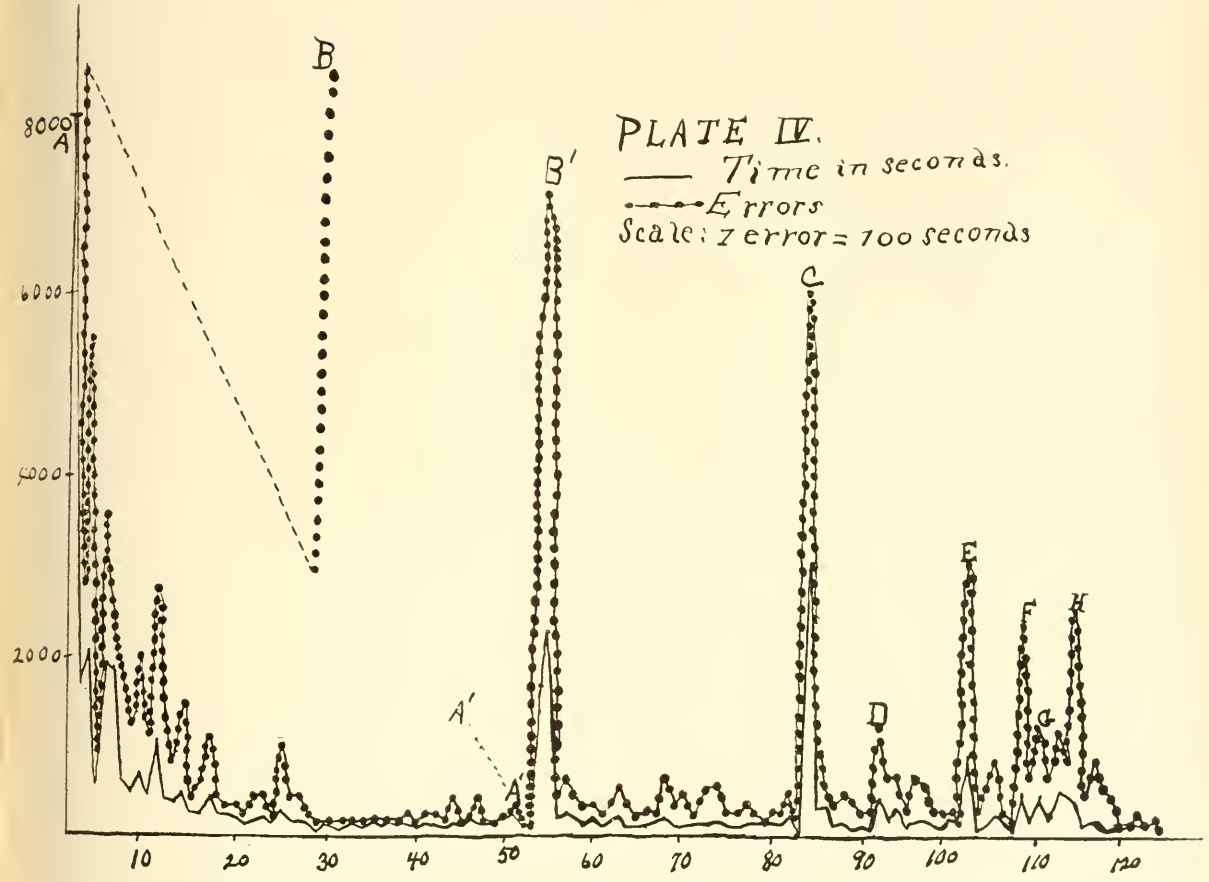
Porcupine No. 3 also had a long and varied experience in the maze. His rate of learning is shown in the first part of curves A and B, plate IV. His first errorless trip was on the 83rd trial and his second and last was the 91st. His constant error was in blind alley 7. He missed 7 only 14 times in 125 experiences with the maze. It ceased to be an error with him as he entered 7 when he was regular and missed it only when he was confused. It was a means of getting past the point *l*. He was accustomed to go into 7 the distance of about twice his length and pivot back on his hind feet, always turning toward the center and always in the same way. It was such a regular part of the trip that one might be disposed to call it an error when he missed going into 7. His "error" was turned to success in the usual habit so that the principle of "trial and success" became as evident as it had been in the puzzle-box tests. Reckoned upon this basis, No. 3 reached his first success on the 31st trial and perfection on the 41st, even excelling the progress of No. 6 but this particular perfection includes regularly a part of blind alley 7 and is thus not entirely comparable. It only

serves to show how soon the animal developed a settled habit by being introduced into the maze only twice each day during the entire summer and fall. Some of the results by No. 3 are reserved for discussion below.

As to the method of the experimenters there is also considerable variation. Kinnaman's (26) monkeys were put through as many times as they would go each day. The same method was used with porcupine No. 6. Nos. 3 and 4 were put through twice daily while Porter's (22) sparrow was allowed to make the trip only once a day. The methods employed by Small (37) and Watson (44) with this pattern of maze are so different that the results are hardly comparable. As results stand, where work can be compared, the porcupines have solved the maze problem more readily than any other animals except the female monkey in the matter of first errorless trip and she did not reach perfection as early as porcupine No. 6.

When one studies the porcupine in its natural habitat one is not surprised at this ability. Kinnaman thought the maze was adapted to the life of the monkeys, since they travel on limbs from tree to tree and no doubt there is a similarity between the problems. The branchings of the limbs present problems very analogous to the runways and cul-de-sacs of the maze. Small and Watson think the rats in creeping around through dark places have a natural experience comparable to that of running the maze. There is no objection to this. No one, however, can follow the natural trails of the porcupine without feeling that these animals thread a maze every time they creep out after food. Their paths, particularly in the fall and winter months, are sometimes worn deep into the ground, winding for miles through the underbrush, ferns, laurels, and shrubbery; they go around large rocks, over smaller ones, under logs and over stone walls, creeping in and out with almost endless branchings, cross ings and shuntings. When the more formal problem of a laboratory is interpreted in the light of this instinctive behavior of the animal the superior ability of the porcupine in maze running may be explained upon a lower basis and in a more natural fashion than is otherwise possible. The suggestion also seems *a propos* even at the risk of repeating, that such tests as the traversing of a labyrinth are not reliable tests of mentality and can in no sense serve as a basis for ranking animals

in a scale of intelligence,—skill in such tests being more closely correlated with instinctive behavior than it is with general mentality.



A—Point where maze was shifted 18 inches.
B—Point where maze was rotated 90 degrees.
C—Point where maze was rotated 180 degrees.
D—Point where maze was rotated 270 degrees.
E—Point where maze was restored to initial position.
F—Maze moved into barn rotated 180 degrees.
G—Maze traversed in darkness.
H—One hundred day memory test.

Rotation of Maze.—Watson (44) sought to determine how his rats learned the maze, as well as what sense they employed in

remembering it, or, what he calls "sense modalities in the process of learning and retaining." By surgical and chemical means, he destroyed hearing, sight, smell and touch, the last, however, as mediated through the vibrissae and the soles of the feet. Still his rats knew the maze after learning it and could even learn it without any one of these senses. He could not, however, destroy all of the senses in the same rat at the same time without destroying metabolism and killing the animal. In eliminating one sense at a time, he still leaves room for the hypothesis that learning is accomplished by the sum of the influence of all the senses. The elimination of one may leave a remainder large enough to be effective without very much deterioration of power. Watson could not, by means of mutilation eliminate the kinaesthetic sense. It was just this sense which the present experiment attempted to modify and which the latter portions of curves in plate IV represent. The maze was situated in the open, on a slope of about 10 degrees with the entrance at the lower side. After porcupine No. 3 had been through 49 times and had attained to the perfection of one error in blind alley 7 and an occasional misstep into 3, the maze was moved 18 inches down the hill, changing all the paths, but in no way effecting their slope or direction. No. 3 was then placed into the maze for the 50th trial. He traversed it with two errors in 2 minutes, 30 seconds whereas his time was usually below one and one-half minutes (A′ plate IV). He moved with a great deal less certainty, but this was most probably due to the change of tactual stimulation of the feet. On the second trip the same day he moved with much more certainty and much greater speed. This greater speed threw him into three blind alleys and slightly increased his time. On the following day, both trips were made with the characteristic turn into 7. Shifting the maze had not materially disturbed his ability.

Before the 54th trial the maze was rotated through 90 degrees clock-wise thus changing the direction and relative slope of every path. The result was that his time increased from 1 minute 8 seconds to 32 minutes 38 seconds and his errors from 1 to 71, (B′ plate IV), and he was so completely lost that he missed 7 for the first time in his experience. The porcupine spent most of his time, on the first trial after rotating, in the first half of the maze. On the next trip his time was in-

creased to nearly 37 minutes. In this test he was lost chiefly in the second half. On the third trip, or the first one the following day, his time was down to two and one-half minutes with four errors; but he did not reach his former ability until the 83d trip, or after 30 trials. This, strange to say, is within one of being the exact number of trials necessary for him to reach the record of a single error in the beginning. It looks like a case of relearning the maze though the curve of relearning is much more regular as will be seen in the figure. Before the 84th trial (C plate IV) the maze was again rotated through 90 degrees clock-wise. The animal was again completely lost in the same maze. This time the porcupine missed 7 and required 51 minutes and 43 seconds to reach the center. On the second trip that day, however, he required only five minutes and attained his former ability in eight trials as compared with 30 trials when the maze was first turned. Before the 92nd test the maze was again turned 90 degrees or 270 degrees from the initial position and the animal was through in seven minutes as compared with nearly 52 minutes when the rotation was made before (D plate IV). He reached his one-error standard in ten trials. The maze was then turned to its original position for the 102nd trial. All went well without an error to *6*. He dropped into *6* but withdrew at once. On leaving that point there occurred what is considered the psychological moment of an important choice. The porcupine on this occasion, hesitated with abortive starts between the forward and backward trail six times. Had he gone right it is reasonable to infer that he would have made his single error, and it would likewise have been a reasonable inference that he knew the maze in the position from the old association. His behavior did reflect earlier experience very strongly, but this time he chose the back trail and wasted nearly 14 minutes in the first half of the maze which he had just previously traversed without difficulty. His time was 14 minutes 55 seconds and his errors 30, but he reached his former record in six trials (E plate IV). Two weeks later with the same maze in the barn and rotated 180 degrees from its last position he found his way in nine minutes and again in three minutes. Similar figures represent the 110th and 111th trips the next day (F plate IV).

Watson (44) found that rotating his maze through 180 degrees

confused his rats and that restoration to the original position again confused them. Rotating 90 degrees had a similar effect but shifting the table and maze so as to change direction of lights and air currents had no apparent influence. In explanation of these phenomena Watson postulates "static sensations, or some non-human modality of sensation." Miss Vincent (44, page 89) in the same laboratory observed that tilting the maze two and one-half inches did not disturb the reaction of the rats to any great degree but this has never been satisfactorily controlled and tested. Small (37) has also pointed out that his rats seemed to have a strong "sense of position." The Peckhams (31) resorted to the same explanation in interpreting the behavior of wasps; but Bethe was probably the first to mention this fact in connection with his observation on the effect of rotating the beehive. The present study so far is defective in that there are two factors entering: (1) The sense of direction, if there be such a thing either organically or through environmental stimuli; (2) the kinaesthetic difference arising from the change in the slope of the paths when the maze was rotated. Either one of these factors would, no doubt, be sufficient to confuse the porcupine and efforts to separate the two have so far been abortive.

Dark Room Experiments.—Another variation was made with the maze in the barn. Porcupines Nos. 3 and 6, both of which knew it perfectly, were used in some dark-room experiments. Windows were blinded carefully and most places where light could enter were closed. In addition, operations were postponed until long after dark so that the room was absolutely dark to the human eye, even after two hours' adaptation. A person who knows the maze well, however, finds no difficulty in keeping trace of the animal's movements even in total darkness. No. 3 was started in the maze in the usual manner. He made four abortive starts in getting past *b*. After passing that point he went wrong in *3* but only for a moment and had a similar experience in *2* immediately after. After that he righted himself and went without error through the remainder of the maze entering the free end near *5* according to his habit without difficulty. His time was 8 minutes 47 seconds, chiefly consumed in the start (G plate IV). His second trip was identical with his first except that he had less difficulty in starting

and consumed a little less time. No. 6 was tried on the same evening but he worked 48 minutes and had not passed the point *b*, having spent his time diligently in the first two arms. The next evening, with no intervening practice, No. 6 was again tried. On second approach he passed *b* and went without an error until he came to *x*. Here he erred, going into a part of the maze which he entered only once previously. Most of his 27½ minutes were spent in the effort to take the long route. His work in this part was not unlike his efforts in a new part when solving it in daylight. After extricating himself and getting on the correct route he had no more trouble. On the second effort in darkness he fell into the same difficulty but found his way out in time to finish the trip in nine minutes.

Memory Tests.—On March 28th, 100 days after these experiments in the dark and with no intervening experience with the maze, or with any other apparatus except the 100-day test with the puzzle-box five days previously, No. 3 was again given the maze in the same position in the barn in which he had last experienced it. He again manifested the tendency to have difficulty in getting started. After wasting 4 minutes 40 seconds and scoring 21 errors in getting past *b* he spent 1 minute in *3* and *2*. When he was successfully past *3* he went without an error from that point to the goal in 50 seconds. His whole time was 6 minutes 30 seconds. Errors 25 (plate IV). The second trip required only 30 seconds to pass *b*. He missed *2* and *3* entirely but dropped into *6* and *7* for short excursions. His time diminished from 6 minutes 30 seconds to 1 minute 5 seconds and his errors from 25 to 4. In each of these trips the animal went the long route, making the difficult turn into the free opening near *5* most perfectly. There was no hesitation at the center and none of the useless clawing on the wires which characterizes the actions of an untrained animal. There was no doubt in the minds of the three experienced observers who witnessed this memory test that the animal retained a very fair mastery of the problem which the maze presents. The behavior of the animal also suggested a strong possibility that motor habit was the dominant factor in the memory reaction, because several times he would start on the right route as if from motor habit and then turn out. The two trips on the following day were made in 1 minute 55

seconds with four errors, and 3 minutes with eight errors. The third day of the memory test brought him to his old standard of a single error in 7 with 1 minute 30 seconds for the trip.

Conclusions from these variations are:

1. The trail of a beaten path, whether it be by smell, or vision, or touch, or all combined is a slight but not an essential factor in the porcupine's ability to follow a known way to a given point.

2. The sense of direction in the porcupine is strong, though probably not playing the rôle of a dead reckoning. The surrounding trees are probably used as visual landmarks if one may judge from the tendency of the animals to work in parts of the maze nearest to the surrounding trees. It has also been observed that when in the natural runways they cross an open space they generally emerged from and re-entered the forest under cover of low-hanging trees and creep beneath all the intervening evergreens.

3. The kinaesthetic sense depending on a complex of muscle, joint, and tendon sensation is the one most effectually modified by rotating the maze on the slope and is, doubtless, one of the most important elements in retention.

4. The porcupine's eye is either adaptable to exceedingly dim light, or they can follow a known path without the sense of sight.

5. Memory of the maze through a period of 100 days with no intervening practice and with little diminution of ability, suggests either that sufficient experience had been given with the new problem to fix the habit or that the problem of the maze is comparable with those on which the survival of the species depends.

RESUMÉ

1. In the foregoing study 16 porcupines have been used. Eight were male, four were female, and in the cases of four the sex was not ascertained. Conditions as near to the natural environment as possible have been maintained at all times by means of outdoor cages, dark dens, natural barks for food, etc.

2. It has been ascertained that porcupines are nocturnal and perennial, showing little or no hibernal tendencies. Their span of life is not known but is probably a decade or more. They usually give birth to but one young each year. Young are

born fully equipped with barbed quills, teeth, claws and the sense organs formed and probably ready to function.

3. There are very few indications that porcupines possess the play instincts usually observable in warm blooded animals or this instinct of activity if present at all disappears soon after birth.

4. Porcupines are very easily tamed. They usually eat from the experimenter's hands one day after capture and were regularly at work within a week. This docility is as characteristic of old animals as of young ones.

5. Porcupines ward off all enemies, except those of their own species with erected quills, by powerful strokes with the tail and by quick side and upward lurches of the main part of the body. In all minor quarrels they meet their own kind from the front and with adpressed quills, showing to that extent a consciousness of kind. They do not throw their quills as darts but all quills are barbed so that they cling to the victim on very slight contact thus giving the erroneous impression that the quills are hurled.

6. The hind feet of the porcupine are well adapted to climbing, or standing and walking erect on a horizontal surface. The forefeet or hands are more mobile and especially adapted to grasping and holding, although the thumb is beneath the surface and the digits move in unison.

7. As a subject in experimental study, the porcupine is steady, industrious, cleanly, and harmless. They are sufficiently strong to use large apparatus and yet so docile that they perform well in class demonstration work. The chief hindrance is the uncertainty of life in captivity though the death rate has not been greater than that found by Hagenbeck in his experience with wild animals, several species of which he has not been able to keep alive till he could get them to his Tiergarten.

8. The natural feeding habits of the porcupine are to grasp food first with the mouth but these habits may be permanently modified so that the animals seize and carry food to the mouth with the hands. When the experimenter reached morsels of food to the porcupines they first tried to grasp it with the mouth. If that were prevented they reached with the hand. In 12 out of the 14 tests with newly captured animals, they acquired the habit of using their right hand if the experimenter used

his right hand, or their left hand if the experimenter reached the food with his left hand. In other words, the response was generally symmetrical with the environment. At the same time, all tests and observations in this study indicate that the untrained porcupine is in perfect bilateral symmetry.

9. The habit, perfectly formed, of taking food with one particular hand may be broken very readily by the substitution of the other hand so that no trace of the old habit remains. To re-establish the old habit is almost as great a task as the initial formation of the habit. This seems to indicate that "first impression" is not an extremely important factor in the learning process of the porcupine.

10. Porcupines may readily be trained to use one hand to take one kind of food and the other to take another kind of food and to change the hand as often as the food being offered is changed. In this, there was definitized response to particular and varied stimuli. In making this adaptation they were guided by some form of visual stimuli—probably brightness of the food morsels.

11. Porcupines readily learn and operate puzzle-boxes of four locks in a series. In doing so, each part was operated in the simplest manner consistent with the general method which chance success and repetition had formed into habit and the whole series was carried through with a minimum of effort.

12. No definite determinations have been made on the porcupine's ability to discriminate tones of different pitch. Vocalizations of the species indicate a fairly wide range of tonal discrimination. The cochlea is also very complex. Observation shows them responding more sensitively to sounds of low vibration rate, such as rustling of leaves or the rumble of distant voices.

13. In the use of forms which were constructed to appeal to the denning proclivities of the porcupine, these animals learned to discriminate the circle from other forms presented pair-wise and eventually to find it when it was presented six-wise. In making the choice of the forms, only a part of the form was ordinarily used as a basis for discrimination. There seemed to be some kind of rudimentary image of the circle sufficiently definite to cause the animal to react negatively to each of the

other forms many times in succession but positively to the circle at the first encounter in an experiment.

14. The brightness discrimination of the porcupine is about 10 shades in Nendel's series of gray papers. Possibly a little finer discrimination can be made at either end of the series but a slightly greater variation seems to be required with the middle grays. It is not certain that they cannot discriminate shades of smaller difference but they do not readily use such distinctions in determining their behavior.

15. The porcupine does not, and probably can not, discriminate and react intelligently to color where colored papers are employed as stimuli.

16. No account of the area of acute vision of the eye of the porcupine has been given in the literature, so far as the writer knows. Even Slonaker's excellent treatise omits this animal.

17. The natural instinct of the porcupines to lay out runways in the fall of the year to particular clumps of trees which they use as feeding grounds in the winter, probably has much to do with making these animals especially expert in threading complicated mazes. Sliding the maze so as to change the footing and destroy the paths indicates that the paths are a result rather than a cause of the animals taking the same general direction to the feeding grounds.

18. Rotating the maze 90 degrees thus changing the direction and slope of each of the alley ways confused the animal. After relearning the second time, another rotation of 90 degrees in the same direction had the same effect but relearning the third time was more rapid. A third similar rotation caused less confusion than the others while restoration to the initial position appears to call out the initial responses. Kinaesthetic sensations were, no doubt, modified in these variations and may be also static sensations, if the porcupine possesses such sense-modalities.

19. After once learning the maze the porcupines were able to follow it in the dark, or when darkness was absolute for the human eye after two hours of adaptation. Learning under these same conditions differs little from learning in daylight.

20. Absence from the maze for 100 days at a time did not materially affect the porcupine's ability to traverse it correctly.

21. Other memory tests of the porcupine show better retention where motor or kinaesthetic factors are characteristic of the response than where the same general motor response follows a choice of sensory stimuli, i.e., ability to thread a maze and operate a puzzle-box is retained better than ability to choose between the forms and the brightness boxes.

Conclusion.—It has been the policy of the paper to be conservative at all times in evaluating the material and particularly so in regard to the qualitative results. It has seemed, throughout the study that occasional faint glimpses, or foregleams, have been caught of a broader conception of the animal mind which may sometime, by some master thinker be evolved. The present state of the science is hardly even that of a mosaic. Methods, even, are as incomparable as results. Yet there is hope for the fulfillment in this field, of the prophecy of President Hall (20) in the wider field of evolutionary psychology. "The material for perhaps the most majestic structure yet reared by science is already quarried. The need of, and call for, a master builder in this field must, ere long, produce the man."

In making this study I have been indebted to all who have in any way come in contact with the work. To President G. Stanley Hall for the liberal financial support which Clark University has given; to Dr. James P. Porter for his constant oversight, earnest co-operation and valuable suggestions; to Dr. Wm. H. Burnham for assistance in planning problems and interpreting results; to Dr. J. W. Baird for helpful criticism of the more technical parts of the discussion. Far more than to anyone else, I owe acknowledgment to my wife who has been more than an assistant in the work. Proper recognition would involve the details of whole sections of the study, comprising thousands of tests wherein I have personally taken little part, beyond general daily oversight, from the time the experiment was fully planned to the final evaluation of results after all tests had been made. In addition to this, most of the drudgery of final copying, arrangement of bibliography, drawing of curves, etc., must be attributed to the same co-operation. I shall, of course, assume all responsibility for crudities in the methods, errors in the work, naïvete in the interpretation, or lack of clearness in the exposition.

BIBLIOGRAPHY.

1. Allen, Jessie. 1904. The associative process in the guinea pig. A study of the psychical development of an animal with a nervous system well medulated at birth. *Jour. Comp. Neur. and Psy.*, Vol. XIV, pp. 293–359.
2. Alsberg, M. 1894. Rechthändigkeit u. Lenkhändigkeit. Hamburg. 32 pp.
3. Audubon, J. J. 1849. The quadrupeds of North America. N. Y. Vol. 1, pp. 277–286.
4. Baldwin, J. M. 1903. Mental development in the child and in the race. N. Y. 496 pp.
5. Brehm, A. E. 1883. Thierleben. Die Säugetiere. Pp. 413–416.
6. Burroughs, John. 1895. Ways of nature. Page 3 and page 58.
7. Buschan, G. H. F. 1909. Menschenkunde; ausgewählte Kapitel aus der Naturgeschichte des Menschen. Stuttgart. 265 pp., pp. 248–251.
8. Breed, Frederick S. 1911. The development of certain instincts and habits in young chicks. *Behavior Mon.*, No. 1.
9. Chamberlain, Alexander F. 1909. Review of Dr. Gould's book on righthandedness and lefthandedness. *Amer. Anthro.*, Vol. 11, No. 3, July-Sept.
10. Cole, L. W. 1907. Concerning the intelligence of raccoons. *Jour. Comp. Neur. and Psy.*, Vol. XVII, May, pp. 211–261.
11. Cole, L. W. and Long, F. M. 1909. Visual discrimination in raccoons. *Jour. Comp. Neur. and Psy.*, Vol. XIX, Dec., pp. 657–683.
12. Craig, Wallace. 1908. The voices of pigeons regarded as a means of social control. *Amer. Jour. of Socio.*, Vol. XIV, No. 1, July, pp. 86–100.
13. Darwin ,Charles. 1873. The expression of the emotions in man and animals. N. Y. Pp. vi+372.
14. Davis, H. B. 1907. The raccoon: A study in animal intelligence. *Amer. Jour. of Psy.*, Oct., Vol. XVIII, No. 4, pp. 447–489.
15. Gould, Geo. M. 1908. Righthandedness and lefthandedness. Phila. and London. 210 pp.
16. Groos, Karl. 1898. The play of animals. N. Y. Tr. by E. L. Baldwin. 341 pp.
17. Hagenbeck, Karl. 1909. Beasts and men. Longmans, Green & Co., N. Y. 299 pp.
18. Hagerty, M. E. 1909. Imitation in monkeys. *Jour. Comp. Neur. and Psy.*, Vol. 19, pp. 337–455.
19. Hahn, Walter L. 1908. Some habits and sensory adaptations of cave inhabitating bats. I–II, *Biol. Bull.*, Vol. XV, July, pp. 135–193.
20. Hall, G. Stanley. Fifty years of Darwinism. Chap. on evolution and psychology. Pp. 251–267.
21. Hamilton, G. V. 1911. A study of trial and error reactions in mammals. *Jour. Animal Behavior*, Vol. 1, No. 1, Jan.-Feb., pp. 33–66.
22. Hicks, Vinnie C. 1911. The relative value of different curves of learning. *Jour. Animal Behavior.*, Vol. 1, No. 2, Mar.-Apr., pp. 138–156.
23. Hicks, Vinnie C. and Carr, H. A. 1912. Human reactions in a maze. *Jour. Animal Behavior*, Vol. 2, No. 2, Mar.-Apr., pp. 98–125.
24. Hodge, Mildred A. and Stocking, Ruth J. 1912. A note on the relative value of punishment and reward as motives. *Jour. Animal Behavior*, Vol. 2, No. 1, Jan.-Feb., pp. 43.–50
25. Hunter, W. S. 1911. Some labyrinth habits of the domestic pigeon. *Jour. Animal Behavior*, Vol. 1, No. 4, July-Aug., pp. 278–304.
26. Kinnaman, A. J. 1902. Mental life of two Macacus Rhesus monkeys in captivity. I–II, *Amer. Jour. of Psy.*, Jan. and Apr., Vol. XIII, pp. 98–148.
27. Livi, Rodolofo. 1908. Sulla causa del destrismo e del mancinismo. *Atti. d. soc. Rom. di Antropol.*, Vol. XIV, pp. 91–94.

28. Mollison, W. S. Rechts u. Links in der Primatenreihe. *Korrespondenzblatt der deutschen Anthropologischen Gesellschaft*, Vol. XXXIX, pp. 115. 1908.
29. Morgan, C. Lloyd. Animal life and intelligence. London. Pp. xvi+512. 1891.
30. Mosso, Angelo. Psychic processes and muscular exercise. Clark Univ. Decennial celebration, pp. 383–395. 1899.
31. Pecham, Geo. W. and Elizabeth G. Wasps: social and solitary. Boston. Pp. xv+311. 1905.
32. Porter, J. P. The psychology of the English sparrow. *Amer. Jour. of Psy.*, July, Vol. XV, pp. 313–346; 1904. April, Vol. XVII, pp. 248–271. 1906.
33. ———. Intelligence and imitation in birds, a criterion of imitation. *Amer. Jour. of Psy.*, Jan., Vol. XXI. pp. 1–71. 1910.
34. Reigard, Jacob. An experimental field study of warning coloration in coral-reef fishes. Papers from Tortugas laboratory of Carnegie Institute. Washington. Vol. II, pp. 257–325. 1908.
35. Romanes, G. T. Mental evolution in animals. Appleton Co., N. Y. 411 pp. 1900.
36. Shiras, George 3rd. A flashlight story of an albino porcupine and a cunning but unfortunate coon. *The National Geographic Magazine*, Vol. XXII, No. 6, June. 1911.
37. Small, W. S. An experimental study of the mental processes of the rat. *Amer. Jour. of Psy.*, Jan., Vol. 11, pp. 133–165; 1900. Jan., Vol. 12, pp. 206–239. 1901.
38. Thayer, G. H. Concealing coloration in the animal kingdom. N. Y. 1909.
39. Thorndike, E. L. Animal intelligence. The Macmillan Co., N. Y. 297 pp. 1911.
40. Washburn, Margaret Floy. The animal mind. The Macmillan Co., N. Y. 333 pp. 1908.
41. Washburn, M. F. and Abbot, Edwina. Experiments on the brightness value of red for the light adapted eye of the rabbit. *Jour. Animal Behavior*, Vol. 2, No. 3, May-June, pp. 145–180. 1912.
42. Watson, John B. Animal education, an experimental study of the physical development of the white rat, correlated with the growth of its nervous system. Univ. Chicago Press. 122 pp. 1903.
43. ———. The behavior of noddy and sooty terns. Papers from the Tortugas laboratory of Carnegie Institution. Washington. Vol. II, pp. 189–255. 1908.
44. ———. Kinaesthetic and organic sensations. Their rôle in the reactions of the white rat to the maze. *Psy. Rev. Mono. Suppl.*, Vol. 8, No. 2, pp. +100. 1907.
45. ———. Literature for 1910 on the behavior of vertebrates. *Jour. Animal Behavior*, Vol. 1, No. 6, Nov.-Dec., pp. 430–447. 1911.
46. ———. See No. 52. Yerkes and Watson.
47. Watson, John B. and Carr, Harvey. Orientation in the white rat. *Jour. Comp. Neur. and Psy.*, Vol. XVIII, pp. 27–44. 1908.
48. Yerkes, Robert M. Animal psychology and criteria of the psychic. *Jour. Phil. Psychol. and Sci. Meth.*, Vol. II, pp. 141–149. 1907.
49. ———. The dancing mouse. A study in animal behavior. N. Y., 290 pp. 1907.
50. ———. Habit formation in the green crab. Carcinus granulatus. *Biol. Bull.*, Vol. III, Oct., pp. 241–244. 1902.
51. Yerkes, Robert M. and Morgules. The method of Powlow in animal psychology. *Psych. Bull.*, Aug. 15, Vol. VI, No. 8, pp. 257–273. 1909.
52. Yerkes, Robert M. and Watson, John B. Methods of studying vision in animals. *Behavior Monograph*, No. 2. 1911.

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CONTENTS

Page

Acknowledgment iv

Part I. Pitch-Discrimination

Historical 1-19
Method of Pavloff's school 1
Goltz's "Gehirnlos Hund" 3
Munk's experiments 3
Kalischer's earlier work 4
Rothmann's experiments 13
Kalischer's later work 15
Swift's experiment 18
Summary 19
Preliminary experimentation 19-27
Problem, animals and method 19
Results 24
Control experiment 27
Conditions of reliable experimentation 30
Further experiments on pitch-discrimination 41
Experiments on discrimination between noises 46
Summary 51

Part II. Habit-Formation

Problem 54
Apparatus 54
Animals 55
Method of training 57
Results 59
Operative proceeding 71
Post-operative tests 71
Retention 74
Control tests 74
Summary 78

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I. PITCH-DISCRIMINATION[1]

HISTORICAL

Several reports of experimental tests on the audition of the dog have been published. Most of them have been made by physiologists, who were interested not so much in the degree of the animal's discrimination as in the localization of the cerebral centers which govern it.

Pavloff's[2] school (notably Selionyi[3]) have attempted the determination of the dog's sensitivity to differences of pitch, by the saliva-reflex method. Yerkes and Morgulis[4] have given a comprehensive description in English of this method. Some of the most important points in their article are set forth below.

The saliva-reflex (secretion of saliva) occurs under two strikingly different conditions: when the glands are stimulated by the specific (chemical) stimuli for secretion; and when the animal is presented with visual, olfactory, auditory or other stimuli, which have been concomitants of direct stimuli of the salivary grands. "The environment of the dog may be said to consist of two sets of properties, the essential and non-essential." Properties essential for a given reaction are those characteristics of an object which "regularly and definitely determine the reaction of the organism;" non-essential properties of the object are "those which only in a highly variable and inconstant manner condition the reaction." The chemical property of food, whereby it acts on the receptors of the mouth of the dog, is an example of the essential class; the brightness, color, etc., are examples of the non-essential properties. Reflex responses to "essential" properties are called "unconditioned" reflexes;

[1] From the psychological laboratory of the Johns Hopkins University.

[2] Pavloff, J. P. The scientific investigation of the psychical faculties or processes in the higher animals. *Lancet*, 1906, pp. 911–915..

[3] Selionyi, G. P. Contribution to the study of the reactions of the dog to auditory stimuli. Dissertation. St. Petersburg, 1907. (In Russian.) Method reported by Yerkes and Morgulis, l.c., below.

[4] Yerkes, R. M. and Morgulis, S. The method of Pavloff in comparative psychology. *Psychological Bulletin*, 1909, pp. 257 ff.

those in response to "non-essential" properties are called "conditioned" reflexes.

The following experimental procedure is followed: "A normally active and healthy dog of vigorous salivary reaction having been selected, the duct of one of the salivary glands—the parotid for example—is exposed on the outer surface of the cheek and a salivary fistula is formed. The wound heals completely within a few days and the dog exhibits no signs of discomfort or inconvenience. Those who have used the method insist, indeed, that their animals are perfectly normal." Devices are applied to catch and measure the saliva secreted in different intervals of time.

In the auditory tests the animal is accustomed to being fed at a certain tone or sound until the saliva secretion begins when the sound is made. Unfamiliar sounds do not provoke the reaction. Selionyi reports that discrimination was established by this method between tones only a quarter of a tone different in pitch. Further difference-diminution than this did not produce such marked difference in secretion: if the two tones were very near together in pitch, the "unfamiliar" one provoked a secretory response, but not to such a degree of intensity as did the "familiar" one. If the "familiar" tone was sounded too faintly, however, the reflex conditioned by it was either very weak or else did not appear. If an unfamiliar tone the pitch of which differed very slightly from that of the familiar tone were given just before the familiar one was sounded, the reflex conditioned by the latter was partially inhibited.

Yerkes has remarked that there are several obvious disadvantages attached to this method: "conditioned reflexes" die out with repeated stimulation; the quality of food given conditions a remarkable variation in the flow; and the rate of secretion is conditioned also by the time-interval between stimuli. To this I would add the suggestion that there may be also a tendency to rhythmical change in the rate and quantity of secretion. Moreover, Selionyi tells us that the animal was affected by the "kind of movements" made by the experimenter, who had to be careful neither to move too quickly nor to hold himself too rigidly quiet. As slight changes of posture are very often involuntary there is room for doubt whether they

were eliminated. That the method itself can be put to practical use and be so improved as to become wholly reliable has not yet been demonstrated. In any case there remains the question whether the results obtained by this method would show the relative importance of the animal's sensitivity to given stimuli, as referred to his behavior under normal conditions. A change in the auditory stimulus great enough to occasion change in the saliva-reflex conditioned by it may be much too slight to occasion a change in the so-called voluntary reactions; or the contrary may be true.

In 1891, Goltz[5] removed the entire cerebrum from one dog, which lived for over eighteen months thereafter under constant observation in the laboratory. At very high tones or at very loud noises the animal would prick its ears or turn its head. If the stimulus was very intense it would even strike at its ear with the forefoot "as if it wished to stop its ear." From these reactions, Goltz denied that the animal was "totally deaf." Munk calls attention to the possibility that the animal was reacting to pain-stimuli and not to tone or even to noise. This suspicion seems to be well grounded.

Munk[6] himself made some experiments with dogs, using nine pipe-organ tones about an octave apart, and taking the animals' ear-, head- and body-movements as criteria of audition. If these movements were no longer made after operation on the cerebrum, he assumed that deafness has been produced. Having extirpated different regions of the supposed auditory center from trained dogs, and having compared the motor reactions following the operation with the former responses of the dogs, Munk concluded that the dog's center for tone lies in the temporal lobe; that perception of the high tones is conditioned by the function of the anterior portion of the center, while that of the deeper tones depends on the activity of the posterior region. While these results are extremely interesting, and valuable if it can be established that the animals reacted only to auditory stimuli, yet the behavior method is open to the criticism that it cannot be used to determine the animal's discrimination between stimuli. Kalischer, whose work we shall pres-

[5] Described by Munk, H. Functionen von Hirn und Rückenmark. Berlin, 1909. Article Ueber den Hund ohne Grosshirn. 1894. Pp. 137 ff.

[6] Munk, H. Ueber die Functionen der Grosshirnrinde. Berlin, 1890. Pp. 113 ff.

ently take up, asserts also that dogs which after operation made no such responses as Munk chose as criteria, could yet discriminate accurately between tones and between chords. Assuming that Kalischer's dogs were reacting only to auditory stimuli—of which fact there is room for doubt—this objection to Munk's method would be fatal.

Kalischer[7] reported in February, 1907, some experiments which he had carried on for the purpose of testing the relation between the temporal cortex and tone-perception. He purposed particularly to test experimentally the conclusion of Munk regarding the specialized function of this area.

The number and ages of the animals used is not reported, but they were chosen from at least six different breeds. Kalischer trained his dogs to take food upon the sounding of a given tone and to refrain from seizing it upon the sounding of any other. The food was either held in the experimenter's hand before the dog or laid on a chair by which the experimenter stood.

The tones were sounded first on the organ used by Munk, which contained nine pipes—the octaves from C_1 to c^7. Later he substituted the piano and still later the harmonium, finding the latter best suited to his purposes.

The daily tests on each animal were arranged about as follows: Kalischer struck a certain tone and as long as it sounded fed the animal bits of meat from the hand. In the first two daily experiments he sounded only the one food-tone, so that he might accustom the animal to being fed at the sound. "From about the third day on," he says, "I struck now and then another tone, and while it sounded I held the bit of meat in the closed hand, so that the animal could not reach it and had to content himself with sniffing at my hand." Then he caused the food-tone to sound again and fed the animal bits of meat, one after the other as long as the sound lasted. In all his tests thereafter, he "struck besides the food-tone other 'Gegentöne' and at the latter prevented the animal from seizing the food."

Kalischer does not say in what order the respective stimuli were given. The expression "now and then (zwischendurch)" is far less definite than one should desire or expect. As previous

[7] Kalischer, Otto. Zur Function der Schlaeffenlappens des Grosshirns. Eine neue Hörprüfungsmethode bei Hunden; zugleich ein Beitrag zur Dressur als physiologischer Untersuchungsmethode. *Sitz. der K. Preuss. Akad. d. Wisschftn.* 1907, pp. 204 ff.

experimenters[8] have pointed out, and as appears in the behavior of every animal which I have used in discrimination tests, the animals quickly form the habit of reacting in a certain rhythm or in a regular order or by taking a certain position, regardless of the stimulus presented. These "position habits" and rhythmic choices are exceedingly hard to break up. If the order of presentation of the stimuli happens to coincide with or approximate the habitual order adopted by the animal, a very high record, or one even of perfect accuracy may be obtained when the animal really has not been affected by the stimulus.[9] And indeed nothing is easier than for the experimenter to fall into a rhythmic order of presenting the stimuli unless he determines beforehand a chance order of presentation for each day's series of trials. This question should be borne in mind while Kalischer's results are being considered, for we shall have occasion to consider it later.

A further criticism may be made at this point. Kalischer writes that the harmonium suited his purposes better than the piano and some other instruments, because with the harmonium it is possible to sustain the tone as long as is desired—or until the animal has reacted. Now, it should not be forgotten that the duration of the stimulus may easily become the basis of the animal's choice. In such an experiment as Kalischer's, when the dog is to react to one tone and is under penalty to inhibit reaction to the others, reaction may not occur immediately following the stimulus. If the animal is timid reaction will probably be delayed. There is a strong temptation, which I have experienced, to sustain the stimulus-tone a little longer if the animal does not react properly or promptly, so as to give him full opportunity to hear the tone. On the other hand the experimenter is likely to become content quickly if the animal does not react immediately to a "Gegenton" and damp the stimulus; whereas the animal might have reacted had the tone been held a little longer. Rothmann, whose work we shall presently discuss[10] tells us that in his experiments the duration of the stimulus-tone was a disturbing factor, although he

[8] c. f., Yerkes, R. M. The Dancing Mouse, p. 111.

[9] An actual record of one of my animals, nicely illustrating this point is shown on page 52.

[10] Rothmann, Max. Ueber die Ergebnisse der Hörprufung an dressierten Hunden. *Arch. f. Physiol.*, 1908, pp. 103 ff.

reports no precautions against it, and does not consider it in interpreting his data. He says: "After three weeks (of training) the dogs reacted with perfect accuracy; only now and then, at a tone of abnormally long duration, a false reaction occurred." [11] Again, speaking of animals bereft of one posterior corpus quadrigeminum, which were hardly disturbed by the operation: "If the tones were sounded for a somewhat longer time, then the dogs came at every tone." [12] To guard against disturbance from this source, care should be taken to make the duration of all stimuli the same at every presentation. Before Kalischer and Rothmann assumed that their animals were discriminating only the pitch of tones, they ought to have shown in control tests that their animals would not react to a "Gegenton" no matter how long was its duration.[13]

Kalischer does not tell us how many trials he gave the animals at each daily test, nor how many trials altogether were required to achieve perfection. Data on both of these questions are highly desirable in reports of behavior experiments. He does say in this report, however, that the daily test on each animal lasted not longer than five or six minutes. In reporting another experiment of similar character he tells us that about fifteen minutes were required to give eight to ten trials[14]. This tallies with the writer's experience. An animal should not be worked much faster than this, as the risk of its becoming "inattentive" is too great. But it is evident that the stimulus cannot be presented over three to five times in the five or six minutes which Kalischer allowed for a daily series. The possibility of varying the order of presentation of food-tone and "Gegentöne" is also greatly limited by the brevity of the series. For this reason it is doubtful whether the animal is given a fair test of discrimination. An animal making random choices only may chance to make most of the first three or four of a series correctly, and yet be unable to maintain accuracy through a varied series of ten to twenty trials. On the other hand, some

[11] Loc. cit., p. 107.

[12] Loc. cit., p. 109.

[13] It may be said here that it is quite possible to make tests of the dog's sensitivity to mere duration-difference of auditory stimuli. The results of such an investigation should prove very interesting.

[14] Kalischer, Otto. Weitere Mitteilung ueber die Ergebnisse der Dressur als physiologischer Untersuchungsmethode auf den Gebieten des Gehör—Geruchs- und Farbensinns. *Arch. f. Physiol.*, 1909.

of my animals, after making three or four wrong choices successively have finished a series of fifteen trials correctly.

To return to Kalischer's procedure: He says, "From the fifth or sixth day on, even if I held the bit of meat in the open hand, many animals would no longer attempt to seize it at the 'Gegentöne.' Thus ever more frequently correct reactions ensued." From this point on the animals were "punished by a light blow on the jaw when they snapped falsely after the food."

Some of the animals were taught to take food at high tones —some at c-2048 d.v.—and others at as low tones as "C_7."[15] The "Gegentöne" chosen at first were those lying as far as possible from the food-tone. After the animals had learned to inhibit reaction to these, others nearer the food-tone were chosen. Kalischer says that the greater the difference between food-tone and "Gegenton" the more quickly the animal learned to discriminate; but that it was possible and not very difficult to train the animal to discriminate between the food-tone and one only a semi-tone removed. We should examine with some care his description of the animals' behavior in the experimental situation, however. "Bei den weitab vom Fresston liegenden Tönen pflegte später der gut dressierte Hund, scheinbar erschreckt, schnell zurückzuspringen, während er bei den näher leigenden Gegentönen *öfter Neigung zeigte, zuzuschnappen*, (italics mine) was sich deutlich an den Kopfbewegungen beobachten liess." Again: "Liess man den Fresston oft hintereinander ertönen, die zunächst prompt nach die Fleischstücken gegriffen hatten, E r m ü d u n g s e r s c h e i n e n geltend." "Die Tiere hörten auf, nach den Fleischstücken zu greifen; und erst wenn man zwischendurch wieder einen der Gegentöne angeschlagen hatte, griffen die Tiere von neuem beim Fresston wieder in gewohnter Weise zu. * * * Auch hier war es von Zeit zu Zeit nötig, zwischendurch einen der Gegentönen erklingen zu lassen."

These remarks indicate that Kalischer regarded his animals as discriminating even though they sometimes did not react to the food-tone and did not inhibit reaction to "Gegentöne." We may then seriously inquire what was the experimenter's criterion of discrimination. How could he know that failure to respond was due to fatigue and is not rather evidence of lack

[15] This is doubtless a misprint: C_1, 64 d.v., is probably meant.

of discrimination? If the reactions and inhibitions are no more definite than this, interpretation of results is extremely unsafe. I have already referred to a day's record made by one of my animals, showing an "accuracy" of 80%, whereas the animal was not being affected by the stimulus. Dependable results cannot be had from less than several fairly long and perfect consecutive daily records, obtained under conditions of control.

A further question: Assuming that the "mental" condition indicated by "scheinbar erschreckt" was legitimately ascribed to the animals, to what may we best consider it due? Since the experimenter was in the room, near the dogs, we are not safe in saying that their "fright" was occasioned by association of only the perceived tone with punishment. The work done by Moll [16] and later by Pfungst [17] and Stumpf on the now famous horse of Herr v. Osten, "*Der kluge Hans*," showed that in a situation of expectant interest it is almost impossible for one to avoid making certain slight, often unconscious and involuntary movements; and it has been demonstrated that animals, hypnotic subjects, gamblers, mediums, and even the most honest laboratory subjects may react unconsciously to such movements, thus accomplishing the deception of the experimenter and sometimes of themselves. It is not proof of actual inhibition of such movements to say as did Shepherd [18] that "care was taken to avoid them." The most important point brought out in the investigation of *Hans* is that the movements are made both unconsciously and involuntarily, and are usually discovered indirectly. In Kalischer's work there is good reason (especially in view of the remarkable results reported) for suspecting the presence of such a factor. The experimenter stood before the dog, being doubtless the center of the animal's interest, ready to give food or to strike as soon as the dog sprang up on to the chair. The punishment had to be administered quickly to prevent the animal from obtaining the food. One would expect that slight, nascent movements involved in giving food or striking would inevitably be made. Besides these and

[16] Moll, A. Hypnotism (4th ed.); tr. by Arthur F. Hopkirk. New York, Scribners, 1910.

[17] Pfungst, Oskar. Clever Hans. Tr. by Carl L. Rahn. New York, Henry Holt & Company, 1911.

[18] Shepherd, W. T. Discrimination of articulate sounds by cats. *American Journal of Psychology*, 1912.

other possible changes of posture (e.g., strain and relaxation of neck- or body-muscles), are possible involuntary changes in breathing, etc., any of which the animal could quickly learn to associate with food or with punishment.

Kalischer indeed attempted two control-tests of the existence of secondary criteria. First, he asserts that he made "some" of his dogs temporarily blind, by sewing their eyelids together. He reports that the accuracy of discrimination was not affected. Now, if this temporary blindness were complete, this result indeed would show that the secondary cues if there had been any, were not visual cues; but it would not show that there were no secondary cues of another kind. Changes of tension in the operator's body-muscles, of breathing, etc., can be detected by other than visual avenues. I lay what may seem undue stress on this possibility, because, as will appear later, there is reason to believe that such cues, involuntarily given, were factors in some tone-discrimination tests which I made on *blind* dogs.

Besides this form of control, Kalischer destroyed one cochlea in some other "well-trained dogs." No disturbance is reported. When the other cochlea was destroyed, all discrimination ceased. Dogs subjected to extirpation of both cochlea before any training was attempted did not learn to discriminate at all. Kalischer regards this as evidence that the *other* dogs had ignored extra-auditory stimuli. The possibility still remains, however, that in the tests given the normal animals, the experimenter expected them to react when the food-tone was sounded, and to inhibit reaction when other tones were sounded; and that he made "unconscious" movements corresponding to his expectation, to which the animals reacted. On the other hand, in testing dogs in which the cochlea had been destroyed, it is possible that the experimenter, believing them to be deaf, did not expect them to react, and hence did not make the movements which one would expect in the opposite situation. The most honest introspection will not enable him to decide whether "unconscious and involuntary" movements were made. If would have been much better if Kalischer had applied some form of control tests with the experimenter and others as well out of the room.

Kalischer, however, was satisfied that the dogs were discriminating between "exclusively auditory perceptions," and on that assumption proceeded to operate on the auditory center as

defined by Munk. The details of his report of the operative procedure are as meager as are those of his behavior tests.

The first operation was the extirpation of one temporal lobe from an animal whose cochlea on the same side had previously been destroyed. According to Munk the *nervi acustici* make a perfect chiasmus, and this operation should render the animal completely deaf. On the same assumption, if the cochlea on one side were completely destroyed and the auditory area on the same side only partially extirpated, deafness to some tones only—high or deep according as the anterior or posterior region were mutilated—should be produced. Kalischer reports, however, that his animals reacted to tones as before, no matter whether extirpation was partial or complete. There was a noticeable disturbance in responses to commands and more or less disturbance of orientation, but no details beyond these statements are given in Kalischer's report.

Before proceeding to extirpate the opposite temporal lobe from these animals, Kalischer allowed from four to five weeks for recovery, during which he continued his training. He observed no change in the animals' discrimination. "Nach der ersten Schläfenlappenextirpation hatte sich kein Unterscheid in den Verhalten der Tiere bei der Dressurversuchen gezeigt. Die Tonunterschiedsempfindlichkeit und der Reactionen der Tiere waren die gleichen geblieben, gleichgültig, auf welcher Seite die erste operation ausgeführt worden war."

The extirpation of the temporal lobes, Kalischer says, followed in general the plan of Munk, but sometimes a larger area was removed, both wider and deeper (three-quarters cm. deep) and sometimes opening the ventricle.

When the second temporal lobe was removed from some animals the visual area also was injured, Kalischer asserts, so that "a part of the peripheral visual field in both eyes was lost."[19] After this operation the dogs no longer reacted to spoken commands, nor did they show by pricking of the ears or movements of the head any sensitivity to loud noises. Later they apparently began to resume such movements at loud noises and very loud commands, but did not learn to discriminate among them. Before the operation the least whistle or call had been enough to

[19] A reliable method of making an accurate test of the dog's peripheral vision would be valuable and interesting. Kalischer does not describe his method.

bring them forth. Even the movements of the head and ears at noises finally disappeared after destruction of the posterior corpora quadrigemina with a needle. The animals' reactions to tones, however, were much less affected. Tests were resumed from three to four days after the operation. Kalischer says that some disturbances of tonal discrimination followed—i.e., the animals did not always react and inhibit correctly. (The percentage of error and the number of trials are not reported.) But he asserts that the animals "undoubtedly discriminated" between their respective food-tones and "Gegentöne," and ascribes the disturbance wholly to the shock of the operation. This may be correct, but evidence is lacking. If the dogs failed to react properly to the different tones the assumption that their failure was due to some other cause than inability to discriminate, is a mere guess.

Kalischer continues, "from the second week on the animals began to exhibit the old relations; they snapped in accustomed fashion at the food-tone and shrank back at the 'Gegentöne,' and even to the tones adjacent to the food-tone." Indeed, the reactions "appeared almost automatic," the animals "attended exclusively to the food" and less than before to surrounding objects, and their discrimination seemed improved rather than diminished. They reacted correctly when chords and discords were sounded if they contained the food-tone. It was possible to retrain even the "most mutilated" dogs to react to a new food-tone and to inhibit reaction to the former one. Further, animals not trained before removal of both temporal lobes could be taught to make the same discriminations, although they required a longer time than did the others, since they were not easily accustomed to being handled and to making definite movements.

Kalischer does not consider the possibility that the dogs had been rendered deaf by the operation, and were continuing their choice of reaction or inhibition of reaction to certain tones on the basis of rhythmic or habitual order of presentation of stimuli, or of secondary, extra-auditory stimuli, although one would naturally suspect that this were the case. He asserts that no other conclusion is left to us but that the dogs were deaf to noises and not to tone. He concludes accordingly that the perception of noise and that of tone are different functions;

and that different end-organs and different centers are involved for each. The center for noise he locates in the temporal lobe, the afferent pathway to which passes through the posterior corpora quadrigemina. The end-organ he leaves indeterminate. On the other hand, he considers that the cochlea contains the end-organs for tone; and that the center for tone is infra-cortical and even below the posterior corpora quadrigemina, since the only known auditory pathways to the cortex pass through them. On the behavior side Kalischer concludes that we must attribute to the dog an exceedingly fine sensitivity to absolute pitch.

That the defects in his behavior method set forth above render unwarranted all the conclusions which he draws from his data, seems to me clearly evident.

In the foregoing pages has been attempted a rather extensive criticism of Kalischer's work. This to some readers may seem overdone. But to me it seems necessary as well as just, because he has set the pattern for subsequent work by other investigators [20] whose methods have been of the same general type. His work has commanded praise from certain physiologists, as well as from students [21] in other fields to whom some of the difficulties involved in a reliable experimental test in audition are evidently unfamiliar. It seems proper here to suggest that Kalischer would hardly have adopted so crude a method and relied so uncritically on the results obtained from its use, had he first familiarized himself with the previous investigations in comparative psychology and animal behavior. That he is unacquainted with modern behavior methods is indicated by his claim to authorship of the food-stimulus method. He would hardly have advanced such a claim had he been familiar with the work done by Lloyd Morgan, Thorndike, Small, Franz, Yerkes and Watson, all of whom and many others had been using the food-stimulus method for years. The method certainly has great antiquity. After this point had been raised against him [22] he repeated the claim in subsequent writings.

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[20] Swift, W. B. Demonstration eines Hundes dem beide Schlaefenlappen extirpiert worden sind. *Neurol. Centbl.*, xxix, pp. 686 ff.
Also Rothmann, l.c., above.

[21] e. g. Bentley, I. M. *Psychological Bulletin*, April 15, 1912.

[22] Franz, S. I. Dressurmethode f. Zentralnervensystemuntersuchungen. *Zentbl. f. Physiol.*, 1907, pp. 583 f. See also Kalischer's reply immediately following. Also Watson, J. B. *Psychological Bulletin*, 1908.

In February, 1908, Rothmann [23] reported to the Physiologische Gesellschaft of Berlin some auditory tests which he had made before and after operations on several dogs, for the purpose of testing Kalischer's findings. It appears that about the time of Kalischer's communication, Rothmann was trying to ascertain the function of the posterior corpora quadrigemina. He was unwilling to accept Kalischer's conclusion that the center for pitch is infracortical and lies even below the posterior corpora quadrigemina. His reasons are: (1) That such an assumption does too much violence to definite localization theories already fairly established; (2) that the dog's discrimination and reactions are undoubtedly very complicated processes, and accordingly, to make them independent of the cerebrum would be practically to abandon all the useful doctrines of the relation of the cortex to "psychical" acts which prevail at present in physiology and pathology. Finally he suggests that in Kalischer's work secondary criteria were possibly not excluded.

He adhered to the following routine in conducting the daily tests on each animal: The stimulus tones were struck on the same organ used by Kalischer and Munk. The experimenter sat behind it "completely concealed by the high pipes, but so that he could observe the dog." "The meat was laid on a footstool before the dog, while the animal scampered freely hither and thither about the room. This had the great advantage that the tone could be struck while the dog was in another part of the room, with his head averted from the food." A *Diener* was posted behind the stool, to drive the dog away when he attempted to take the food except at the food-tone. Rothmann had suspected that Kalischer's dogs, which after the destruction of both corpora quadrigemina and the consequent cessation of all signs of attention to other noises yet continued to behave correctly at tones, may have been reacting to "unconscious helps," as had *Der kluge Hans*. He substituted the *Diener* for the experimenter at the feeding-place, apparently considering that the untrained *Diener*, who, however, must know the food-tones, was less likely to give the animal "unconscious helps" than would be the experimenter, who was "interested" in the

[23] Rothmann, Max. Ueber die Ergebnisse der Hörprufung an dressierten Hunden. *Arch.* 10 *f. Physiol.*, 1908, pp. 103 ff.

problem. As a matter of fact, perhaps unconsciously, Rothmann exposes the weakness of his assumption. He says: "If a wholly disinterested stranger, were introduced for the giving of the food, then it developed that the dogs undertook to get the food at *first even without a tone, or at wrong tones,* but only in the beginning. As soon as they had accustomed themselves to the change, they did their work correctly." (Italics mine.) Here it is evident that they were not disturbed by *fear* of the stranger. The simplest explanation is that, missing the first *Diener's* threatening signs, they reacted at random until they became used to those of the new attendant.

Rothmann does not report the following facts: (1) The number of animals used; (2) the number of daily trials; (3) the order of presentation of stimuli; (4) the duration of the respective stimuli; (5) the position of the animal with reference to the food when the respective stimuli were given; and (6) the experimenter's criterion of discrimination.

The importance of (1) and (2) is self-evident to the experimental behaviorist; that of (3) and (6) has already been pointed out in the comment on Kalischer's work; and Rothmann's admission of the disturbing effect of (4) on his own work has already been quoted. The importance of (5)—the position of the animal when the stimulus is given, is very great, as it may easily become a basis of reaction. As illustrative of this point, the reader is referred to page 53 of this work for a record of a control test made by the writer.

Besides training to tones, Rothmann trained some of his animals to come for food at the words "*Komm her,*" and to refrain from coming at the words "*Kopf scher,*" both combinations being sung on the same tone. Reaction to other words of command was also taught.

The following operative and post-operative procedure was followed: Extirpation of both posterior corpora quadrigemina was performed on four dogs. Three had been previously trained. After the operation, two of these three were trained for about a month to discriminate c-1024 d.v. from the other c's on Rothmann's organ, but without success. The fourth animal, previously trained to react only to c-1024 d.v., and which had learned the problem "faultlessly," was again subjected to the training process beginning four weeks after the operation. Three

weeks were spent in training to the same tone without success; following which twenty-three days were spent in the endeavor to teach the animal to react to the words "*Mach schön*," with the same outcome. From Rothmann's brief account one gathers that the animal reacted when noises or tones were made but did not discriminate among them. Post mortem showed total destruction of the posterior corpora quadrigemina in all the animals.

Extirpation of both temporal lobes in five dogs produced deafness to both tone and noise when the entire area described by Munk was removed; if the removal was not complete, some traces of the reactions remained. A sixth dog, however, not previously trained, having been deprived of both temporal lobes and of one convolution of the *gyrus sylviacus*, was "successfully trained" in three weeks to react only to c-256 d.v. Seven days sufficed to perfect reaction to the words "*Nimm Fleisch.*"

In two dogs not previously trained, both internal geniculate bodies were destroyed. Neither could be trained to respond to either tone or noise. From these results Rothmann concludes that the dog's auditory center lies in the temporal region, but that it extends over a wider area than that defined by Munk. According to him the pathway from the end-organ passes through the posterior corpora quadrigemina and the internal geniculate bodies.

The anatomical findings thus announced are certainly less revolutionary and less spectacular than are those of Kalischer, and conform fairly to the generally accepted view. It should be remembered, nevertheless, that Rothmann's experimental procedure is as unreliable as Kalischer's, if indeed not more so.

Kalischer published in 1909 a report[24] of continued work done on reactions of dogs to musical tones. His conclusions from his former work had left the end-organ for tone indeterminate, and he wished to test the theory of Helmholtz regarding the function of the cochlea and the vestibular apparatus. In this same report he gives account of its extension to olfactory and color-vision tests on the dog. Apropos of the last mentioned part of his work it might be remarked that Kalischer's

[24] KALISCHER, OTTO. Weitere Mitteilung ueber die Ergebnisse der Dressur als physiologischer Untersuchungsmethode auf den Gebieten des Gehör—,Geruchs- und Farbensinns. *Arch. f. Physiol.*, 1909.

apparatus is subject to several gross defects which had been eliminated in color-vision tests on animals made by Yerkes some years before Kalischer's work was published, by the use of the food-stimulus method.

The auditory tests reported in this article were carried on by the same method as was employed in the earlier work, except that some animals were trained to respond to as many as three (single) food-tones—high, middle and deep. Different animals responded to tones from F_2 to f^3, sounded on the harmonium. Kalischer says that in the beginning of this experiment when the food-tone was sounded he helped the animals perform their reactions; but that later they performed them voluntarily "without any help being given." Most of the animals had already been trained to react to one food-tone, and Kalischer tells us that the training had to be continued long enough for the new food-tone to become "fixed in memory" before reaction became sure. How long a time was required he does not say.

After the animals had been trained to two food-tones, one high and the other deep, one labyrinth was completely destroyed, making the animal wholly deaf on that side. The method used was the mastoid opening of Heidenhain. Destruction of both cochlea and vestibular apparatus was made complete. Training was continued for two or three weeks after the operation, which had not damaged the dogs' accuracy in discrimination. By the same method the second cochlea was then exposed, and the part of the cochlea desired was removed by first piercing the "knee-capsule" covering the cochlea with a fine drill-point, and removing the parts with a needle. One animal in particular, trained before the operation to respond only to food-tones A_1 and c^3, suffered no loss of accuracy in discriminating between these tones and all others. Post mortem showed entire destruction of one cochlea, and removal of the other as far down as the lowest turn. Only this small portion of the cochlea and the vestibular apparatus were left intact. The part of the organ of Corti and of the membrane of Reissner contained in this part of the cochlea, and also the cells of the spiral ganglion which belong to this turn of the cochlea, were uninjured. Reaction to the spoken words "*Sechs*" and "*Drei*" was also perfect, the animal being allowed to take food when they were spoken.

In another animal post mortem showed that only the lowest turn of the cochlea, with the part of the organ of Corti and of the ganglion cells contained in it, was destroyed. The parts lying in the middle turn of the cochlea, however, were atrophied, and the vestibular apparatus was slightly damaged. This animal, which had been trained to react only to B_1 and c_3 as food-tones, and to spoken words, and from which previous to this operation the other cochlea had been removed, had reacted both to tone and to noise as had normal animals.

From other animals, one cochlea having been removed, one side of the remaining cochlea was also extirpated, leaving the other side intact, and not injuring the vestibular apparatus. These after the operation did not react to their food-tones until helped. They had lost their "absolute" sensitivity to pitch, says Kalischer, but could still be made to discriminate between food-tones and "Gegentöne," even if the difference was only half a tone. In other animals in which the vestibular apparatus was more or less injured by this operation, disturbances corresponding to the degree of injury were observed. That is to say, they could not be made to differentiate between tones lying close together in pitch, but could differentiate between tones lying farther apart.

From these data Kalischer concludes that the theory of Helmholtz and others that the different parts of the cochlea and of the basilar membrane act selectively as receptors of long or short sound-waves, is untenable. Also, that the vestibular apparatus possesses an auditory function, and is necessary for pitch-discrimination. Further, that all clang-analysis takes place "in the peripheral end-organs of the nervus acusticus."

In an *Anhang* to this part of his report Kalischer says that some of his dogs were brought to discriminate between trichords as well as between simple tones; for instance, an animal trained to react to e^1 and to inhibit reaction to $e\flat^1$, would also react to the chord $c^1e^1g^1$ and inhibit reaction to the chords $c^1e\flat^1g^1$, $c\sharp^1e\sharp^1g\sharp^1$, $d^1f\sharp a^1$, etc. He further says that by the use of a mouth harmonica he was able to demonstrate in experiments conducted in the stable, that the ass also possesses sensitivity to "absolute pitch." The total time required for this demonstration was about one and one-half weeks.

Swift [25] who was interested in the "psychical" processes involved in the reactions of Kalischer's and Rothmann's dogs, as well as in their localization theories, trained two female dogs "after the method of Kalischer," to discriminate between c^1 and e^2, sounded on trumpets. The c^1 was the food-tone. Fourteen days sufficed to perfect the reactions. The number of trials a day is not given. No control tests showing that the animals were not reacting to other than auditory stimuli are reported. A month of rest was allowed before operating.

On the first dog, extirpation of the left temporal lobe was performed. This, according to Swift, produced right hemianopia. He does not say how this fact was determined. The reactions to tone were undisturbed when the tests were resumed, three days after the operation. Ten days later the right temporal lobe was also extirpated. This, Swift says, rendered the animal's blindness nearly total, and also produced left hemiplegia. Discrimination between the tones was not disturbed. The same operations were performed on the second dog, but both lobes were removed at once. She, too, discriminated as unfailingly as before. This, Swift thinks, demonstrates that the center for pitch cannot lie in the temporal lobe. He does not agree with Kalischer, however, that the center for tone can be infra-cortical. He argues that the dogs' reactions involve a complex "intellectual process," and reveal a well developed "ability to think:" hence, that the cortex must be involved. He believes, therefore, that the center lies in the cortex, but outside the temporal region.

If Swift followed the method of Kalischer, as he asserts, then the animals could have reacted to many other cues than auditory, as has been shown in the remarks on Kalischer's first experiment. Nor is it safe to assume on the results of casual tests that a dog is or is not suffering from defective vision. Extensive observation of the reactions of blind dogs to controlled stimuli, some data of which are included in a later part of this work; and tests by standard methods on the vision of normal dogs, made by Haggerty and by myself, have yielded results quite at variance with the popular attribution of visual keenness to the dog. Further it should be said that Swift's

[25] Swift, W. B. Demonstration eines Hundes den beide Schlaefenlappen extirpiert worden sind. *Neurol Centbl.*, xxix, pp. 686 ff.

animals may have been reacting to pain and not to tone; an e-640 d.v. trumpet-tone, sounded at close quarters in a small room, is certainly both loud and high enough to occasion pain in the human subject. And finally, it is a commonplace that Swift's ascription to the dog of "intellectual" processes and a highly developed "ability to think," is unscientific. There is no possible means by which we may experience an approximation to the dog's "content," in the first place; and besides, we can interpret his behavior adequately and far more surely without relying upon a construct. Indeed, we can explain his reactions quite satisfactorily on the assumption that he has no mental content; and the assumption can no more be disproved than can the one of Swift's.

As appears in the foregoing discussion of work done by other investigators, the problem of localization of the auditory center has been left by them in an unsatisfactory state. Rothmann and Munk place the auditory center in the temporal region; Munk asserts that different portions of the region perform highly differentiated functions. Kalischer insists that the center for pitch is infracortical; that tone-discrimination is made in the "peripheral end-organs of the *nervus acusticus;*" and that the center for noise is another center than that for pitch. Swift tells us that the center must be cortical, but that he has demonstrated that it cannot lie in the region pointed out by Munk and Rothman. As has been pointed out, the methods of conducting behavior tests employed by all these experimenters are decidedly crude and their widely divergent conclusions may be explained at least partially by reference to the methods of controlling the animals' behavior.

PRELIMINARY EXPERIMENTATION

In April, 1910, at the suggestion of Professor Watson, I began as an experiment in comparative psychology a series of tests on the audition of dogs, in the hope of accomplishing the following purposes:

1. To see whether the behavior results of Kalischer and others could be confirmed by tests made under more reliable conditions of control.

2. To devise a satisfactory method of testing the limits of pitch-discrimination in the higher vertebrates.

3. To find the difference-threshold for pitch in the dog.

4. If preliminary results should justify the attempt, to repeat the tests on animals on which after training, extirpation operations had been performed by a competent surgeon, and attempt to interpret the disturbances which might result in the light of the post mortem examinations.

The statement may properly be made now that neither of the two goals last mentioned has as yet been reached.

The animals used were two females, litter sisters, mongrels, littered June 15, 1906. At about their second day of life a surgeon in the state hospital for the insane at Wards Island, New York, had assured the continuance of temporary blindness as long as might prove desirable, by first scarifying the edges of the lids and then uniting them with stitches. This caused the upper and lower lids to grow fast together before the ninth day, at which time the puppy's eyes ordinarily open. During the preliminary experiment described herein the dogs were still in the blind state. When this work was begun they had been in the laboratory for over a year but no experiments of consequence had been made with them. Both were laboratory pets and very affectionate, but rather nervous in strange situations.

It seemed best, in order to obtain decisive results, to present the animal with at least fifteen stimuli at each daily test. If a small number—say five, is chosen, variation in the order of presentation is too greatly limited; and if the animal is nervous at first the record is not a fair indication of his discriminatory work.

The animals used in this experiment were fed once a day—at the time set for the experiment, which was early in the afternoon. No food was given until the day's work was begun. If the animal appeared unduly eager to begin work one or two bits of meat were usually given before the stimulus was presented, in order to make the dog better contented. After each day's series each animal was allowed to eat as much as she desired at the time. The food was scrap meat, thoroughly cooked by boiling, and mixed with stale bread soaked in its liquor. Milk was also given from two to seven times a week. Both these and the other animals used in later work remained in splendid condition throughout the experimental work.

It seemed better also to adopt a different test of discrimination from that used by Kalischer, Rothmann and Swift. This, as has been mentioned, was to allow the animal to take food at one or more tones and to refrain from it at others. If an animal is hungry and eager to obtain food, one would expect a tendency to react to the mere striking of any tone; while if the animal is timid there may be a tendency to inhibit reaction, even though the tone were "recognized" as the food-tone. So, to the two tones used as stimuli, two reactions quite different were chosen; to the deeper tone the animal was trained to react by placing her forefeet on a chair at the operator's left, and waiting there for food; to the higher tone, by mounting a low box at the operator's right, and "sitting down" on it until fed. Between stimuli the animal sat on the floor, at the experimenter's feet. Food was given after a correct reaction had been chosen. In case of incorrect choice she was recalled without being fed unless she was unusually nervous, in which event occasionally she was allowed to perform the correct reaction and take food, the reaction being recorded as an error. The problem was considered "learned" when the animal had performed three successive daily series of reactions without error.

The two stimulus-tones chosen were middle c (256 d.v.) and the g above (384 d.v.). The tones were sounded at first on two standard tuning forks, mounted on wooden resonators, and struck by hand. The forks were placed close together on a shelf in front of the experimenter, and rested on heavy cotton felt pads. Their relative position was frequently changed. Later in this experiment the tones were sounded also on several Stern variators, large and small, blown from a Stern tank, and carefully tuned each day to the tuning forks. The merits and defects of this apparatus will be discussed later in this paper. Each tone was sounded until the animal had reacted—usually not more than one and one-half to two seconds.

For about six days, the animals, both of which were seemingly frightened when the forks were first struck in their presence, were "put through" the proper reactions—to one tone several times and then to the other for the rest of the series. As soon as they showed a tendency to react voluntarily they were allowed to work without consciously given help, at least, from the experimenter. Records were taken from this point on.

Four problems were given:

1. Discrimination between the two tones sounded on tuning forks struck by hand.

2. Discrimination between the two tones sounded on the blown variators.

3. Discrimination between the two tones sounded on forks and on large and small variators indifferently.

4. Discrimination between chords containing one and the other stimulus-tones, respectively.

The results are summarized in the following table:

Problem	Dog	Began	Finished	Days worked	Trials required for learning
1	1	4/19	5/ 7	19	285
	2	4/18	5/14	27	405
2	1	5/12	5/19	8	120
	2	5/19	5/24	6	90
3	1	6/ 1	6/11	12	150
	2	6/ 1	7/18	40	600
4	1	8/ 6	10/ 1	41	615
	2	8/ 4	9/30	44	660

The tables showing the daily percentage of error are found on pp. 24ff. The longer learning-time of Dog 2 in experiment 3 was due to disturbance by the falling of a piece of apparatus duing an experiment. She refused to work for nine days and then resumed responses of any kind only after a great deal of coaxing and petting.

Care was taken to sound the tones with varying degrees of intensity. The tones of the variators can be made faint or loud at will by increasing or lessening the diameter of the opening of the air-valve leading to each pipe. Such a change produces also a change of pitch. The latter must be corrected by re-tuning to the fork. Further the same stimulus-tones can be blown on any one of two or three variators. By this means tones of the same fundamental pitch, but of quite different timbre, were given. It was thought that this might prevent the animal from reacting to constant differences of intensity and timbre. The relative position of the different resonators was also frequently interchanged, to prevent possible localization from becoming a factor.

In experiment 4 the following chords were used: (1) Containing c′ (256 d.v.) c′-e; c′-e′c″; g-c′-e′; c′-eb′; c′-eb′-c″; g-c′-e♭′; a-c′; f-c′; f-c′-a′; a-c′f′; c′-a′-f′; (2) containing g′ (384 d.v.); g′-b; g′-b′-g″; g′-b♭′; g′-b♭′-g′; e′-g′-c″; e♭′-g′-c″; g′-b′-d′; d′-g′; d-g′-b′; g′-b′-d″. These chords were used indifferently, regard being paid only to which of the two stimulus-tones was contained in the chord. That the chord might be sounded at once, an extra valve was put on the main pipe leading from the tank, which was to be kept closed except when the stimulus was given. Thus the stops opening the valve leading to each variator could be pulled out as desired, and when the main valve was opened the entire chord would be sounded at once.

The results of this experiment approximate closely those obtained by Kalischer and Rothmann. I have never observed, however, what they both report, namely, that after a few trials have been given the animal becomes "fatigued." If the animal is left alone as many as thirty trials may be given in control tests, evoking a prompt response each time. This makes me suspect that the failure of Kalischer's dogs to react to the food-tones, if the latter in a four or five minute series were "allowed to sound often one after the other," was due to a lack of discrimination rather than to mere fatigue.

A control test of retention was given sixty days after the last problem had been "learned." Following a private discussion of Kalischer's contention that the dog has an "exceedingly fine sensitivity to absolute pitch," (of which, however, I have never been convinced) I invited the members of the psychological journal club to witness a test of the animals' ability to react properly to their old stimulus-tones. The animals had not worked on the problem since it had been discontinued—in fact, they were then engaged in learning to open a problem-box for their daily food. The two stimulus-forks used in the first problem were used and struck as before. Each dog was given eight stimuli, in the order indicated by one of the observers watching the experiment through a glass door. Each dog reacted without error. Dog 1 was sniffing at the place on the floor where her problem-box usually stood, and when the first fork was struck, merely crouched and kept sniffing hastily at the floor where she stood. When after perhaps two seconds the fork was struck again, she again crouched and in some confusion found

her way to the chair and mounted it properly. All her other reactions and all those of Dog 2 were prompt.

TABLE 1

DISCRIMINATION BETWEEN FORKS C-256 D.V. AND G-384 D.V. STRUCK BY HAND

	Dog 1	Dog 2
Day	% Accuracy	% Accuracy
1	47	40
2	53	33
3	60	47
4	40	73
5	40	40
6	47	47
7	53	53
8	60	53
9	80	47
10	66	47
11	86	67
12	73	47
13	86	80
14	80	67
15	93	87
16	93	80
17	100	87
18	100	87
19	100	93
20		93
21		80
22		87
23		100
24		87
25		100
26		100
27		100

TABLE 2

DISCRIMINATION BETWEEN VARIATORS C-256 D.V. AND G-384 D.V.

	Dog 1	Dog 2
Day	% Accuracy	% Accuracy
1	80	80
2	73	80
3	80	93
4	86	100
5	86	100
6	100	100
7	100	
8	100	

TABLE 3

DISCRIMINATION BETWEEN C-256 D.V. AND G-384 D.V. STRUCK ON FORKS AND VARIATORS INDISCRIMINATELY

Day	Dog 1 % Accuracy	Dog 2 % Accuracy
1	47	60
2	40	66
3	53	20
4	47	00
5	73	00
6	73	00
7	87	00
8	100	00
9	100	10
10	100	00
11		20
12		33
13		47
14		40
15		66
16		80
17		80
18		47
19		40
20		60
21		53
22		53
23		73
24		66
25		73
26		80
27		80
28		66
29		73
30		87
31		87
32		80
33		93
34		80
35		93
36		93
37		93
38		100
39		100
40		100

TABLE 4

Discrimination Between Chords Containing c-256 d.v. and g-384 d.v. Sounded on Variators

Day	Dog 1 % Accuracy	Dog 2 % Accuracy
1	67	73
2	47	60
3	67	47
4	67	60
5	73	80
6	73	60
7	60	87
8	73	87
9	47	80
10	73	87
11	80	87
12	73	80
13	87	80
14	80	87
15	80	53
16	87	80
17	53	67
18	47	73
19	73	80
20	80	73
21	87	87
22	87	73
23	100	60
24	67	73
25	80	73
26	87	80
27	87	87
28	80	87
29	80	87
30	87	80
31	73	53
32	87	60
33	87	80
34	80	80
35	93	80
36	93	80
37	93	93
38	100	47
39	100	73
40	100	93
41		87
42		100
43		100
44		100

Although these results taken at their face value seem to confirm those of Kalischer and Rothmann, yet they were not wholly satisfactory. The experimenter had been in the room, near the dogs, throughout the experiment. While he was not yet trained to observe and to guard against the giving of less noticeable secondary cues, yet on casting the results it was easy to recall many times when an animal had changed her reaction if the operator happened to turn his head, shift his body-weight from one foot to the other, or catch his breath. Also, that if the stimulus-tone was not damped as soon as the animal had selected her feeding-place, she would sometimes go hastily to the other. Further there was room for doubt whether the order of presentation had been sufficiently irregular, as in the records of the animal's work on the first two problems, there were shown only the number of right and wrong reactions respectively to each stimulus-tone, the order of presentation not being given. These facts being considered it seemed better not to rely on the results at hand until other tests could be made in which these possible disturbing factors were eliminated.

Accordingly in the summer of 1911 the same two animals were subjected to control tests, which, through the kindness of Professors Angell and Carr, were performed in the laboratory of the University of Chicago. The two stimulus-tones chosen were middle c-256 d.v. and e′-320 d.v. It was believed that the animals would quickly learn to discriminate between these and that other tones could then be introduced, to which yet different reactions could be made.

In these tests the stimulus-tones were struck on standard forks by an assistant[26] who sat in a room twelve feet away, separated from the animal-room by two partitions, one of which was a nine-inch brick wall. A wooden tube four by six inches in cross-section was used to convey the sound into the animal-room. Between stimuli the animal was confined in a cage just under the sound-pipe, and when the stimulus-tone was struck, was released by the experimenter's pulling a string from where he stood, in the room adjoining, by which act the door of the cage was noiselessly released. In the opposite corner of the

[26] This part of the experiment was conducted with the assistance of Mr. J. W. Shields, then a graduate student in the University of Chicago, to whom my thanks are due.

room were two food-boxes, each above a chair which the animal was to mount with her fore-feet. The two chairs were fastened with sides together, and with their fronts secured to the wall, so that the animal had to face west in mounting the chair to be associated with the tone of the c-fork; and east in mounting the one to be associated with the tone of the e-fork. When proper choice was made food was dropped on to the chair from a chute above, which was opened by the pulling of a string in the hand of the experimenter. In case of incorrect choice the animal was recalled without being fed. The reactions were observed by the experimenter from without the room, who watched the work through a hole in the wall three inches square.

In the beginning of this experiment the animals were first given each tone separately, and "put through" by the operator. After eight or ten stimuli had been given the animal showed a tendency to go to the box of her own accord. After each animal had performed fifty consecutive reactions to the c-fork correctly without being put through, she was trained to go to the other box at the striking of the e-fork and allowed a like number of reactions. Then both stimuli were given irregularly in a given series, and the animals' unaided reactions were recorded. Each early developed a "position habit," at first usually choosing the box where she was last fed or avoiding the one from which she has last been recalled without being fed. Then followed in each dog a preference for one box or the other, causing its selection from 70% to 90% of the chances. Then each dog acquired a rhythmical habit, going to one box, now to the other, regardless of the stimulus presented. To break this up the animal was worked against her preference—the order of presentation being so arranged that the animal's method would seldom secure food for her. Later both animals reacted irregularly, but as appears in table 5, with little regard to the stimulus given.

After 37 days—505 trials each, the experiment had to be interrupted. There was little evidence of discrimination in the dogs at the end of the test.

There were some indications, however, that they might be failing to react correctly, not because of inability to discriminate, but because of lack of "attention." If on leaving the cage the animal immediately began a wide detour to the right or

left in going to the food-box, the reaction was nearly always correct; but if she started by a middle path toward the boxes, which were near together, the reaction was incorrect as often, at least, as correct. It was usually possible for the experimenter to predict from the animal's breathing when she was about to become indifferent. This stage was observed sometimes in the beginning of a series, sometimes only about the middle, and sometimes near the end. It could hardly be interpreted therefore, as fatigue, especially since the animal would always make *some* choice very quickly. The "delayed reaction" factor

TABLE 5

DISCRIMINATION BETWEEN STRUCK FORKS C-256 D.V. AND E-320 D.V.
(Control experiment, Chicago)

	Dog 1	Dog 2
Day	% Accuracy	% Accuracy
1	00	10
2	00	00
3	00	00
4	33	40
5	40	47
6	40	73
7	27	40
8	53	10
9	60	40
10	67	53
11	73	40
12	27	53
13	33	33
14	53	53
15	33	73
16	30	27
17	30	33
18	60	47
19	80	33
20	50	60
21	70	40
22	70	60
23	70	53
24	50	53
25	20	40
26	40	40
27	40	53
28	60	10
29	50	20
30	60	53
31	30	90
32	40	53
33	60	60
34	80	53
35	80	53
36	30	53
37	60	60

is evidently present here, and the experimenter was unwilling to accept these negative results as conclusive until the tests had been made under such conditions as would compel the animal to make her choice very soon after the stimulus is presented.

From these preliminary experiments it becomes evident that in order to reach reliable results in the field of pitch-discrimination new apparatus must be constructed and such conditions fulfilled as will enable the experimenter to exclude certain vitiating factors inherent in tests like my first ones, and of the Kalischer and Rothmann type. An enumeration of these defects, with apparatus proposed for their elimination, is set forth in the section next following.

THE CONDITIONS OF A DECISIVE EXPERIMENT ON PITCH-DISCRIMINATION IN ANIMALS

It is probably evident that neither in the results of my own preliminary experiments nor in the work of the other investigators which has been discussed, can we be at all sure that the dogs were reacting only to auditory stimuli. Some obvious secondary cues have to be eliminated from the experimental conditions if results of future experiments are to be reliable. It seems worth while to mention these disturbing factors, some of which others have previously pointed out in describing discrimination-experiments of other kinds.

1. "*Unconscious helps.*" By this term is meant any sort of body-movements, which the animal can learn to associate with a definite reaction. Those likely to be made by the experimenter under such conditions as Kalischer's and Swift's, or by the assistant in Rothmann's experiment—namely, such nascent movements of the arms and body as would accompany readiness to strike or to step back to allow the dog to obtain food, are particularly vicious, as appears in the "Clever Hans" report. They can be detected visually by many animals. But visually sensed aids by no means exhaust the list. Suppose the operator has been in the habit of recalling the dog when a wrong choice is made, or of scolding him if he fails to inhibit, and encouraging him by a word if he is timid in reacting, when the problem is like Kalischer's and Rothmann's; a very slight change in

breathing will be noticed even by a blind animal. There are other movements of like kind, which serve as auditory stimuli. Now, there is only one reliable means of preventing disturbance from these sources: *Not only the experimenter but all others should be outside the room in which the animal is working.* Rothmann's *Diener* or any bystander can disturb the dog as much as could the experimenter, and indeed is more likely to do so, as the experimenter if he is honest is apt to take greater pains to be on his guard. Meeting this condition does not preclude the experimenter's putting the animal through the proper reactions in the first two or three days' work, although it is doubtful if this is desirable; but as soon as the animal is left to discriminate, the experimenter should leave the experiment room. In control tests, even though he be in another room, it is highly expedient that *the experimenter should not watch the animal while making his choice*, but wait until the choice is made. This will prevent the animals being given any auditory "unconscious helps." In my later work I observed this precaution, with satisfactory results.

2. *The order of the presentation of stimuli.* This must be varied irregularly. In a given series of say 100 presentations, the animal should be given 50 of each. In actual training work, I have found, as have also Yerkes, Watson and others, that it is usually unsafe to give the same stimulus more than three times in succession, as a position preference is apt to become established. In control tests, after the animal has really learned to discriminate, the number of successive presentations of the same stimulus can be safely increased. The best method I have used in training work for determining the order of presentation is the use of a well shuffled pack of cards; allowing those of one color to represent one stimulus, and those of the other color the other stimulus. If more than three cards of the same color appear in succession the extra ones can be laid aside until needed to break into an overlong series of the other color, or until the bottom of the pack has been reached. This method assures the variation being irregular, and eliminates the possibility of the animal's successful reaction merely to the order of presentation. The probability of the coincidence of an order so determined with the animal's established preference, is extremely slight. As has been suggested above, the experimenter

making up an order of presentation at random may too easily fall into a rhythm of his own.

3. *Duration of the stimulus.* This should be made as nearly the same for both stimuli as possible. If the animal reacts quickly the stimulus tone may be sounded until choice has been made; if he reacts slowly, however, the chances are that he will disregard the stimulus if its duration is much more than one or two seconds. This may work injury if the animal habitually "sets himself" to react when the stimulus has ceased—as some animals will do. If two tones widely different in pitch are used, the tendency of the higher one to die away more quickly than the deeper, may be a disturbing element; if the animal reacts slowly, both tones should be damped at the end of the time allowed, regardless of the animal's behavior.

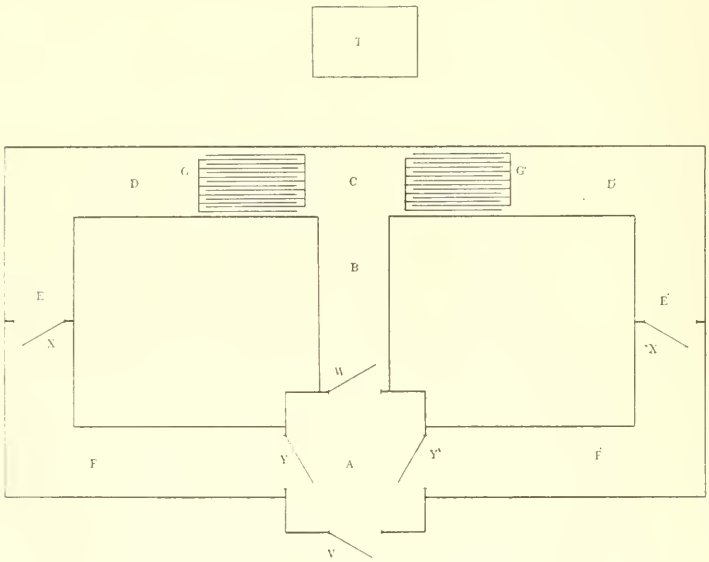

FIGURE 1—A, home-box; B, introductory alley; C, opening into alleys D and D'; E, E', alleys entering food-compartments F and F'; G, G', punishment grills; T, table containing stimulus-forks. V, W, X, X', Y and Y' are doors, automatically swinging in the direction indicated. The experimenter's place is in an adjacent room by window O in front of door V of the home-box.

4. *Position of the animal when the stimulus is given.* This should also be the same for all stimuli—or as nearly the same as is possible. I have already referred to a control-test of my own which illustrates the value of this precaution. This condition may be met by the use of the stimulus-cage which I have devised, a cut of which appears on page 32. The explanation of the sketch is as follows: A is the home-box, 4′ x 4′, in which the animal is placed between stimuli, entrance being made from without by the door V; B is an introductory alley, 6′ long and 2′ wide, leading to the alleys D and D′. These alleys are each 10′ long and open to alleys E and E′, which are shut off by the two doors X and X′. These doors, as appears in the cut, are made to open from the animal, and are closed automatically by a small coiled spring. Each is provided with an iron lift latch, which should be heavy enough to catch when the door is closed without attention from the operator. A string is fastened in a small hole drilled in the end of the lever of each of these latches, and run through an eye-screw in the door above, then through a pulley attached to the side of alley E or E′ as the case may be, then to the operator's place, so that by pulling the string the door may be unlatched and pulled open without the operator's leaving his place. When the string is released the door closes and latches itself. Alleys E and E′ open into two food-compartments F and F′. The covering of these boxes is provided with two doors, located near the end of alleys E and E′, through which food is dropped. Y and Y′ are two doors opening from food-compartments F and F′ into the home-box A. These doors are not provided with latches, as they close behind the animal, flush with the jambs, and cannot be opened by an animal which has not free use of its hands, such as has the monkey, raccoon, or the squirrel. They are provided with coiled springs, like those on doors X and X′. My animals would usually open these doors from the food-compartments, merely pushing their way into the home-box, but it is well to provide a means of opening them with strings as are doors X and X′, for the sake of timid animals. Door W is opened by means of a spring, as doors X, X′, Y and Y′ are closed. A heavy gut cord is fastened to an eye-screw near the top of door W, and run through a small hole near the top of the outside frame-work of home-box A, to the operator's station, where it is hooked until door W

is to be released. G and G′ are two punishment-grills—strips of brass about 3′ long, secured to a white pine board 3′ x 2′. Alternate strips are connected with the respective poles of the secondary coil of an inductorium, leaving the other end free. When the current is switched in, the animal's foot must rest on two or more of these strips, which are only1 cm. wide and 1 cm. apart, thus completing the circuit and causing the animal to receive a shock. The inductorium should be placed outside the room in which the animal is being worked, and far enough away that the sparking noise will not disturb the experiment. The current may be shifted through G or G′ by a double-throw switch at the operator's station. My experience showed that in a cage of this size the grills G and G′ should be made longer than three feet; six feet would be much better. Some of the dogs used in these experiments would jump over the grill when they were shocked, instead of turning back. They probably would not have persisted in this attempt had the distance been greater. As it was, additional rods had to be run from side to side of alleys D and D′, above the grills, so that the dogs could not jump over them and escape punishment.

The frame-work of this cage is constructed of yellow pine, 1″ x 3″; the top and sides are covered with woven steel wire, having a mesh about 1 cm. square. Food is kept in both the food-compartments F and F′. The animal is given the problem of choosing a turn to the left into alley D, leading to food-compartment F, at one tone, and a turn to the right into alley D′, leading to food-box F′, at the other tone. The stimulus-tone may be struck while the animal is in the home-box A, and the animal released after it has been damped; or the tone may be sounded after the animal's release, say one-half second before he can reach the end of the introductory alley. If punishment is to be administered, the animal receives it instantly he makes the wrong turn.

In a part of the writer's experiments presently, to be described the experimenter sat at a table about four feet from door V of the home-box. When the animals should have begun to discriminate a screen could be interposed between the table and the cage to conceal the operator. Later the entire appa-

ratus was moved into another room, and the experimenter remained entirely outside the animal-room until after the animal had reacted and been admitted to the proper food-box. The animal was observed through the center window shown in Fig. 1.

Besides the elimination of the secondary criteria mentioned above, it is also desirable to eliminate another possible source of disturbance, namely the lapse of attention which may ensue if the animal's reaction is delayed. This may be accomplished by giving the stimulus very shortly—say within a second, before the animal's choice is to be made. The optimal time may vary with different animals, but it should be possible to control it. In using the stimulus-cage described above the stimulus may be presented at any time desired before the animal reaches the turn into alleys D and D'.

The criterion of discrimination should not be less than a perfect record, maintained through at least three days. The arbitrary standard of 95 perfect trials in the last 100, which some investigators of other kinds of discrimination have adopted, may be allowable; but certainly a poorer record will not suffice. The higher standard is preferable to this.

The final condition which must be fulfilled is the controllability of the stimulus. It is well known that musical tones differ not only in pitch, but also in intensity, timbre, non-musical concomitant noise, and perhaps in localization. These other characteristics must be controlled if we are to make the assumption legitimately that the subject is discriminating on the basis of pitch alone. In addition to this desideratum, it is also desirable that the conditions under which one experimenter works may be reproducible by another, that results may be comparable.

At the present time no satisfactory means is known of measuring the intensity of sound. It may be varied quite widely, however, in electrically actuated tuning forks and in blown pipes and bottle whistles; the latter vary in pitch with the quantity of air admitted. The method of varying the intensity of the stimulus in forks will be described later in this section.

The best means of controlling timbre is by the use of instruments which will give tones as nearly pure as possible. Reed instruments cannot be controlled in this respect. Tuning forks weakly actuated give tones as nearly pure as can be had, although even in these a high anharmonic partial can usually

be detected. Weakly blown pipes or bottle whistles are perhaps the next best. If nearly pure tones cannot be obtained, the next best plan is to have the tone of the same fundamental pitch sounded now on one pipe or bottle-whistle, now on another which is larger or smaller than the first. Thus tones of the same pitch but of quite different timbre can be produced. If the animal, after being trained to a tone on one instrument, shows disturbance when another instrument giving the same tone is substituted, it is safe to say that he was not reacting merely to the pitch of the tones, and that other characteristics were more or less prominent.

The same may be said of non-musical concomitant noise. A struck fork gives a peculiar "cluck," which is rarely the same in any two; a bowed string may have a peculiar scrape which lends individuality to it; a blown pipe or whistle always gives a whisper, which may be a part of its individuality; while the rattle of no two reeds is perhaps exactly the same.

A tone comparatively pure cannot be localized correctly in a closed room because of standing waves; its apparent localization shifts with the observer's position. Angell [27] asserts that it cannot be localized even in the open air. If pure tones cannot be made, the position of the stimulus instruments should be frequently changed in control tests. High overtones and non-musical concomitant noises may be localized quite easily by an animal, and serve as the basis of discrimination.

Of instruments which will fulfill these conditions, one's choice is limited to tuning forks and blown pipes or bottle-whistles, preference being by far on the side of the first. A reed instrument, such as the harmonium or the mouth-harmonica, which Kalischer used, does not admit of control of timbre, nor of satisfactory control of intensity, and the accessory noise is considerable. Trumpets, which were used by Swift, are not certain in pitch, and the timbre varies greatly with the pitch and intensity of the tone sounded.

If blown pipes or whistles are used they should be easily tunable, as the daily change in temperature and density of the air affects their pitch considerably. Differential organ pipes are fairly satisfactory. The Stern variators also meet this

[27] Angell, J. R. Localization of tone. University of Chicago decennial publications, Chicago, 1900.

requirement. The vernier scales and the card-board index glued on the dials of the variators, however, are useless and should not be relied on. There is wide difference between the marks on the scale and the actual pitches under variable atmospheric conditions; and the play of the cogs in the device for raising and lowering the piston, is in some cases greater than three points on the scale.

If pipes or whistles are blown from an ordinary Stern or Whipple tank they are not accurate in pitch within a limit of about 3% in either direction, as the air pressure is not constant. For preliminary or rough work they are often useful when blown in this way, but should not be depended on for finer work. The Stern tank is much too small. It will not blow a large whistle or a c-256 d.v. pipe. The air is so quickly exhausted that if it is in the same room with the animal, the noise of raising it is often disturbing. If chords are blown on variators blown from it, it requires filling after nearly every stimulus. For finer work Watson [28] has devised and installed in the psychological laboratory at the Johns Hopkins University an air-system consisting of a tank filled by a motor-driven positive pressure blower. The tank supplies air-streams from a distant room. The pressure is remarkably constant, and makes possible an accuracy of pitch in blown pipes or whistles far beyond that hitherto attainable. The apparatus is rather expensive.

The apparatus adopted for finer discrimination work of this kind is a system of "tandem-driven" tuning forks similar to that recommended and used by Helmholtz [29]. In the present tests the apparatus was arranged as follows: A c-64 d.v. fork is mounted in a room 100 feet distant from the experimental room for electric actuation. A diagram of this fork and its equipment is shown in Fig. 2. The electrical connection is as follows: From the positive pole of a six volt two ampere storage cell to a rheostat, thence to the pole a, through the fork and platinum contact p with the mercury cup c, to the magnet m, to the pole b, to the negative pole of the storage cell. Thus with each vibration of the fork the current is made and broken at the contact p with the mercury cup c. The mercury in c

[28] Watson, John B. Article as yet unpublished.
[29] Helmholtz, H. v. Sensations of Tone (Ellis' translation).

is kept covered with alcohol, and a secondary circuit through a condenser is made by connecting at pole a and the mercury cup c. This is to eliminate the noise of sparking.

In the experiment-room, on a table, T, about six feet from the end of alley B of the stimulus-cage shown in Fig 1, are placed the stimulus-forks. A diagram of one is shown with its mounting in Fig. 3. The ones mounted for this work are c-256 d.v., e-320 d. v., g-384 d.v. and c-512 d.v. As will be seen these vibration-rates are all simple multiples of that of the primary fork. The stimulus-forks are not provided with contacts but are mounted with magnets between the prongs. The current through these magnets is made and broken at each complete vibration of the primary fork, and the impulse thus given is sufficient to actuate the stimulus forks. The wiring is as fol-

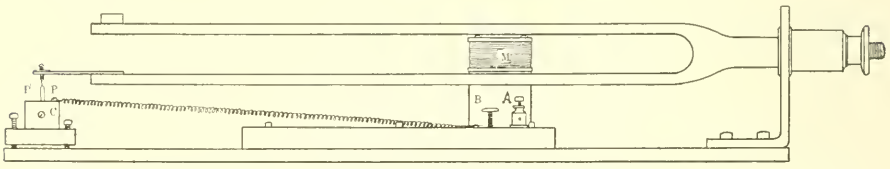

FIGURE 2—A, B, binding posts; C, insulated mercury cup; P, P′, platinum contacts; M, magnet.

lows: From the positive pole of a four volt six amperes storage cell to contact a on the mounting of the primary fork, through the fork and platinum contact p′ to a second mercury cup not shown in the cut of the primary fork, but which is separated and insulated from mercury cup c by hard rubber mounting; thence to a rheostat, through the magnet coil between the prongs of the stimulus-fork, to a double-throw switch at the table at the experimenter's station; thence to the negative pole of the storage cell. The primary fork is kept going throughout the daily series; and the experimenter has only to turn the double-throw switch to cause either of the two stimulus-tones to sound.

Above the stimulus-fork shown in Fig. 3 will be noticed a König resonator. This mediates only the pure tone of the stimulus-fork. It is kept in place by a sleeve which is fastened to the upright rod on the fork's stand. A tone of maximum intensity is obtained when the resonator opening is about 1 mm from the ends of the prongs of the fork. The intensity can be

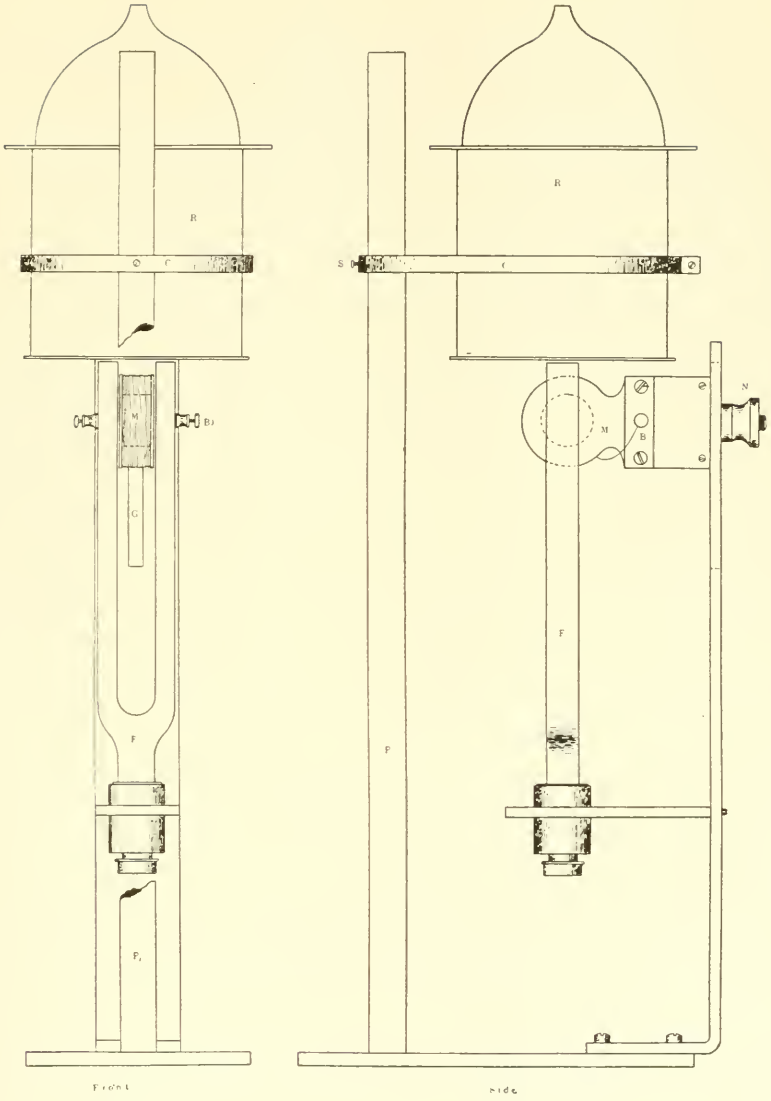

FIGURE 3A FIGURE 3B

F, standard fork; R, König resonator; C, clamp; S, set screw; P, pillar supporting R; A, B, screw contacts; M, magnet, supported by nut, N, sliding through groove G in back wall.

varied widely by varying the distance between the resonator and fork. A vernier scale may be placed on the sleeve and the

rod, which shows this distance accurately. Then if the amount of current passing through the magnet of the stimulus-fork be known and the distance of the resonator from the fork be also known, it will be possible to duplicate these conditions and convert these readings into absolute units of intensity in case a means of measuring it satisfactorily should ever be perfected.

If a wooden resonance case is used instead of the König or Helmholtz resonator, results are not so satisfactory. Intensity can indeed be varied by varying the amount of current passing through the magnet of the stimulus-fork, but if the current used be strong, overtones become quite prominent. Besides the fundamental tone of the fork, there are also present a high anharmonic partial and an undertone of the pitch of the primary fork. The latter is caused by periodic increases in amplitude of vibration of the secondary fork, which synchronize with the vibration of the primary fork, due to the impulse of the current made and broken by the primary. This undertone, however, seems to be mediated by the wood of the table and of the resonance case. If the latter be removed and a König resonator substituted for it, and if the stand on which the stimulus-fork is mounted be padded heavily with cotton batting, then this undertone is not detectible by the human subject, even with the aid of a resonator. Under the same conditions the high partial cannot be heard by the best human observers at a distance exceeding two feet, so it probably does not work much disturbance. No overtones can be detected by use of the ordinary resonators. The maximum intensity of the fundamental is very great.

By this means may be obtained a tone which is practically pure, of widely variable if not measurable intensity, incapable of being localized by the human subject at least, and free from accessory noise.

Through the use of such a system and the stimulus-cage described above, I believe that it is possible to meet all the conditions of reliable tests on pitch-discrimination in animals which I have enumerated above: the elimination of unconscious helps; the elimination of the "delayed reaction" factor; the use of a decisive criterion of discrimination; and the controllability of the stimulus.

FURTHER EXPERIMENTS ON TONAL DISCRIMINATION WITH IMPROVED APPARATUS

In the fall of 1911, the experiments on pitch-discrimination were continued in the Johns Hopkins laboratory, with the improvement in method just suggested. The two tones to be discriminated remained c-256 d.v. and e-320 d.v. They were sounded on "tandem-driven" forks mounted with König resonators, as described above. The reaction to the c-fork was a turn to the left at the end of alley B, and the choice of food-compartment F; that to the e-fork, a turn to the right and choice of food-compartment F′. The experiments had to be conducted at night—from 10 to 2 o'clock, when a quiet building could be had. In addition to Dogs 1 and 2, two normal female puppies, littered June 1, 1911, of which Dog 2 was the dam, were introduced as a control. These animals are referred to hereafter as Dogs 3 and 4.

The first two or three days of work was spent in feeding the animals in the stimulus-cage, and getting them accustomed to passing through the doors without hesitation. After they had become apparently "at home" in the new environment, each animal was "put through" the proper reactions to the two tones for three series of fifteen trials each, and then left to work out the problem for herself. Punishment was not introduced until the twenty-first day of training. The results were not satisfactory. The animal's reactions were greatly retarded in every case, and a certain disturbance resulted which none of the animals entirely overcame. A shock too weak to be disagreeable or even to be perceived when applied to the human subject's dry hand, would often cause great disturbance in an animal which had ignored it for several series. Care was always taken, too, to weaken the current still more if the dogs' feet were wet.[30]

For several weeks Dog 1 would go to grill G′, mount with the left foot the sill on her right, and reaching under the left fore-leg with her right forepaw, would scratch the grill vigorously for as long as five minutes, sometimes, barking furiously all the while. The stimulus-tone was being sounded continuously—or at intervals of one second from the time she was

[30] Breed, in his work on vision in the chick minimized changes in shock conditions by having the floor of the home-box covered with moistened material so that the animals' feet were *always* wet. This precaution seems well taken.

released from the home-box. After prolonging this behavior, she would suddenly proceed or turn back and choose the opposite alley. Strange to say, some of her best "accuracy" records were made during behavior of this kind. Dogs 3 and 4 insisted on leaping over both grills from the beginning—even before punishment was introduced. Later, rods were thrust through the meshes of the wire forming the sides of the alley, which compelled these dogs to creep under them and walk across the grills. Both these animals usually consumed some minutes in hesitation before choosing, after punishment was introduced, and

TABLE 6

DISCRIMINATION BETWEEN C-256 D.V. AND E-320 D.V. SOUNDED ON TANDEM-DRIVEN FORKS IN STANDARD CAGE

Day	Dog 1 % Accuracy	Dog 2 % Accuracy	Dog 3 % Accuracy	Dog 4 % Accuracy
1	20	67	20	60
2	30	70	00	40
3	30	50	70	70
4	20	30	70	50
5	10	70	30	50
6	30	40	60	40
7	20	70	50	40
8	20	70	30	60
9	70	40	20	50
10	30	50	20	20
11	60	47	60	53
12	40	70	30	30
13	40	60	60	40
14	20	30	30	40
15	40	60	30	50
16	40	60	40	60
17	70	90	30	70
18	80	50	30	60
19	60	60	50	10
20	47	53	30	30
21	53	66	33	73
22	53	60	53	53
23	67	67	40	47
24	60	67	40	47
25	40	73	50	70
26	20	53	50	73
27	33	33	50	60
28	60	40	40	10
29	40	33	47	67
30	33	60	20	53
31	60	67	60	46
32	73	60	53	47
33	80	33	40	53
34	47	40	80	40
35	80	66	33	47
36	50	46	53	80

TABLE 6—Continued

Day	Dog 1 % Accuracy	Dog 2 % Accuracy	Dog 3 % Accuracy	Dog 4 % Accuracy
37	66	46	73	53
38	40	40	40	40
39	60	53	53	67
40	53	53	60	46
41	53	60	53	47
42	53	53	40	60
43	73	60	80	40
44	67	73	60	60
45	67	60	60	47
46	53	60	80	60
47	40	73	53	47
48	73	67	47	47
49	73	53	67	53
50	73	47	53	53
51	60	67	40	53
52	47	53	73	47
53	47	53	40	73
54	33	40	40	66
55	47	33	33	60
56	53	26	53	23
57	47	47	47	33
58	33	20	47	80
59	33	53	53	80
60	47	47	67	60
61	67	33	47	40
62	60	33	73	40
63	80	40	73	73
64	60	46	80	73
65	53	20	60	67
66	47	53	53	67
67	60	53	80	53
68	80	60	60	60
69	80	47	60	40
70	67	53	80	60
71	40	47	47	40
72	40	73	53	53
73	40	60	60	67
74	67	47	40	73
75	60	40	80	73
76	60	47	53	40
77	60	53	40	60
78	67	60	40	67
79	47	40	60	47
80	60	40	53	47
81	53	73	67	53
82	47	60	73	47
83	80	53	60	47
84	47	40	53	47
85	80	53	73	73
86	60	53	40	53
87	60	60	40	47
88	67	47	47	73
89	80	60	53	60
90	47	60	80	40
91	67	53	73	40
92	60	53	53	10

Dog 3 developed a variety of position habits. After perhaps three weeks both became accustomed to conditions so that they did not hesitate in choosing the alley to a food-box; but neither could be made to turn back immediately a shock was received. Both would proceed to door X or X′ as the case might be, or cross the grill at least, before turning and going to the proper place. Dog 2 was little affected by punishment.

The learning records, showing percentage of daily accuracy, for ninety-two days of this work are shown in table 6. Not only was the problem not learned in that time, but no animal showed promise of improvement.

In the belief that the two tones may have been so nearly alike that the dogs could not discriminate, the experiment was abandoned for the time, and the animals were given the problem of associating release from the home-box at the stimulus-tone c-256 d.v. with food in food-compartment F, and release from the home-box without a stimulus-tone with food in food-compartment F′. Ten days (150 trials) was allotted for this work. There was no change in the dogs' behavior after the problem had been changed, beyond a return for one or two days to old position-habits which had been abandoned. At the end of ten days, as appears in the learning table below, discrimination was not established, nor was any improvement shown by any animal. This was taken as indication that the animals had been disregarding the stimulus-tones entirely. The results of this experiment are shown in table 7.

TABLE 7

DISCRIMINATION BETWEEN C-256 AND *No tone*

Day	Dog 1 % Accuracy	Dog 2 % Accuracy	Dog 3 % Accuracy	Dog 4 % Accuracy
1	40	40	60	53
2	53	47	60	53
3	67	60	47	47
4	53	53	60	47
5	60	53	47	60
6	47	47	40	50
7	67	53	20	00
8	40	40	60	60
9	47	40	47	60
10	40	67	53	67

The explanation of these unexpected results is not easy. It is of course possible that in ordinary noises the dog is stimulated

only by the high overtones, which in tones as nearly pure as those sounded on this apparatus, may have been lacking or so faint as not to be effective. There is of course, no proof of this but the point may be found worthy of future tests. These are not practicable at the present time.

It has been suggested by a critic that the stimulus chosen—a pure tone—is one so different from those stimuli to which the animal is provided with some form of instinctive response, that the animal should not be expected to learn readily to respond to it. Inasmuch as a pure tone cannot be localized, there may be some force to this suggestion. It seems to the casual observer at least that most of the auditory stimuli which affect the dog are significant because they are noises which can be localized and quickly associated with the things making them. However, admitting this point, the fact remains that it is useless to try to test any theory of localization of the center for pitch by experimenting on the dog, unless the animal can be made to discriminate on the basis of pitch-difference alone. This fact can be established only by the use of pure tones as stimuli, and by working under some such conditions as are proposed above as a standard. It is also open to question whether discrimination could not yet be established if by proper means the animal could be made to receive the stimuli.

The suggestion was also made that in the stimulus-cage the animal is in a "highly artificial" situation, and reacts under emotional constraint. This point is made much of by Shepherd, also, in his discussion of experiments in vision [31].

The truth of the first half of this suggestion is of course patent. Any experiment made under conditions of sensory control is necessarily "artificial." The animal which turns at the flutter of a bird in the leaves is probably affected by visual, olfactory and possibly still other stimuli than auditory. These stimuli taken together with the sound form the "situation" in which the animal is, and are readily associated with the response, which is more or less instinctive. The additional auditory stimulus may be only a "contributing agent," which makes the action of the others effective. The animal may be compared to the subject in a reaction-time experiment, prepared to react

[31] Shepherd, W. T. Mental processes of the Rhesus monkey. *Psychological Monographs*, 1911.

when the click or flash comes; and if we may speak for the moment in terms of consciousness, the degree of "attention" to the auditory stimulus itself is minimal. In the stimulus-cage, however, since the choice of reaction must be made on the basis of a single characteristic of the tone, the demands on his "attention"—at least until he has formed many new associations—are necessarily much greater than in the "natural" situation. Accordingly for a time at least his behavior should be expected to be quite different from what it would be in the "natural" situation. But there is no need for lamentation over this condition; it is imposed on every experimentor in sensation or sensory responses in which results are to be trustworthy.

As to the other half of this suggestion—that the animal reacts under emotional constraint—I do not feel so generous. Certainly some emotional disturbance attends the giving of punishment and I am by no means convinced that in working with an animal as highly organized as is the dog, it is desirable to use punishment. But once the animal has become accustomed to his daily work the signs of "emotional constraint" are few. The dogs which I have used are always eager to work; the excitement which attends the experience probably compensates in large measure for hunting and other instinctive reactions, which the animal in captivity cannot make. My dogs on being admitted to the experimental room certainly perform the reactions which a hunting dog makes when presented with a gun, and which are commonly interpreted as "signs of pleasure." [32]

EXPERIMENTS ON DISCRIMINATION BETWEEN NOISES

The point admitted in the discussion of the first part of the last mentioned criticism, may seem to some to strengthen a criticism which has been made: namely, that there is a hiatus

[32] I feel that it is hardly necessary to consider the suggestion which has been made, that the dogs which I used, being mongrels, should be expected to prove less sensitive to pitch-difference than blooded dogs; and further that acuity of audition varies greatly in different breeds. This certainly is Kalischer's report, but as has been shown in the remarks on his method, this result is no more reliable than the rest which he reports, as it is not evident that his animals were reacting to tone. I know of no other data offered as experimental evidence. But admitting the fact, the criticism is without point as applied to this work. My dogs used in the preliminary experiments showed the same apparent discrimination as did Kalischer's, Rothmann's and Swift's; perhaps greater, because they actually made *perfect* records for three days in succession. The control tests indicate that this discrimination was only apparent.

in my results; that the discrimination shown by the animals in the preliminary experiment might not have been lost had the experimenter gradually effaced himself. In other words: It may be too much to expect of an animal that he form the association between a sound and food in a certain box without some helps in the beginning at least; although he may be able to form the association with help and yet retain it after the helps have been eliminated.

It is manifest that the justice of this criticism cannot be settled *a priori*. Accordingly it seemed well to determine whether, and if so, how readily an animal thrown on his own resources can to discriminate between auditory stimuli alone. As pure tones were clearly out of the question for the time being at least, it was decided to use non-musical noises, which might present to the dog differences in other characteristics than that of pitch.

These experiments were conducted with the same animals and in the same stimulus-cage used in experiments 6 and 7. It had been removed meanwhile to a building at Homewood, which was more quiet than the one in the business section of the city could be. The experimenter was never in the room with the animals when the stimulus was given, but gave it from the room adjoining, where he could not be seen. After the animal had become started on the problem the experimenter did not even watch her while in the act of making her choice, but waited until after he had heard her cross the punishment grill. Thus the question of "unconscious helps" is eliminated. The order of presenting the stimulus was predetermined by the use of a well shuffled pack of cards.

The stimulus-noises were sounded on two ordinary electric buzzers. In the first experiment, designated as problem 8, one buzzer was placed over each of the two doors X and X′, but were merely hung over the edge of the cage by their flexible wire, without touching the cage itself. This was to lessen the probability of the animals reacting to vibration of the cage. The buzzers were actuated by current from two ordinary dry cells connected in series; the current being made by the operator pressing a simple contact key. Being placed over the entrance-doors to the food-compartments, they could be easily and in-

fallibly localized by the human subject. There was a noticeable difference in intensity, that of buzzer 1, placed in this experiment over door X, being considerably the louder. The quasi-tones were also of different pitch, that of buzzer 2, over door X′, being near the third of buzzer 1. This selection was made deliberately. The timbre of the two respective sounds also differed. The sound of buzzer 1 was decidedly nasal, while that of buzzer 2 was quite brilliant. Since these conditions unfortunately are not reproducible elsewhere this description should suffice. In a word, the two stimuli differed in frequency, amplitude, form and direction of the sound waves.

Contrary to the method employed at the beginning of the experiments on tone-discrimination, the animals in this experiment were not "put through" at the beginning, but left to form the associations between a particular sound and a particular food-box for themselves. Punishment was not given in case of incorrect choice. Care was taken to make the duration of each stimulus one-half second, or as near one-half second as possible.

The problem assigned the dogs was the association of the sound of a given buzzer with choice of the food-compartment over which the buzzer was placed. For the first two days after the animals were introduced to the problem each dog tended to react negatively to the stimulus. This was followed, except in case of Dog 4, by a tendency to choose food-compartment F′ regardless of the stimulus presented. This was broken up on the third day of the experiment by sounding buzzer 1 a second time after the animal had wrongly gone into alley D′. After only two or three repetitions this produced a returning into alley D. It was not continued after this day, however, as the experimenter feared that the animal might make it, rather than the actual sound of the respective buzzers, the cue for reaction. The learning tables, which follow, reveal a situation quite different from that of the tone-discrimination problem.

This experiment shows plainly that the dog can learn very quickly and without help to discriminate between two auditory stimuli. The question remains whether the discrimination in this case was on the basis of pitch, intensity, timbre or localization. During the experiment the animals often pricked their ears and turned their heads toward the sounding buzzer, so it

TABLE 8

DISCRIMINATION BETWEEN BUZZERS

Day	Dog 1 % Accuracy	Dog 2 % Accuracy	Dog 3 % Accuracy	Dog 4 % Accuracy
1	67	47	53	47
2	60	60	47	60
3	73	87	47	80
4	87	80	73	93
5	73	100	67	93
6	87	93	80	93
7	93	100	87	100
8	87	100	100	100
9	60†	100	100	100
10	100	67*	100	
11	100	100		
12	100	100		

seemed evident that they were localizing the sound, at any rate. It seemed well, therefore, to ascertain what effect interchange of the buzzers would have on the dogs' reactions. In this control-experiment, designated as problem 9, buzzer 1 was placed over door X′ and buzzer 2 over door X. The sound of buzzer 1 was to be associated with food in compartment F, and that of buzzer 2, with food in compartment F′, as in problem 8, just learned. The difference in conditions was that food was now to be obtained in the compartment *opposite*, instead of at the sounding buzzer.

Each dog was continued for five days on problem 8, which had been learned some three weeks earlier. The animals had done no work meanwhile, but the feeding hour had remained the same. Each animal had given at least three successive days of perfect records immediately preceding the beginning of problem 9. The first day's record of each animal on problem 9 immediately follows. The letters R and L signify right and left compartments, respectively.

This control test shows quite clearly that the location of the source of sound with respect to food is the characteristic of the stimulus which had been determining the animal's reactions. Each dog had been simply going to the compartment

* The problem was "learned" on the ninth day. The record of the tenth day is of a control-experiment, described on page 53, made to show the disturbing effect of variable position of the animal when the stimulus is given.

† The animal's work on this day was disturbed by the falling of a heavy door near food-compartment F while the series was in progress. For the remainder of the day she refused to choose F under any conditions. She was left to run freely in the cage all night in order to overcome the disturbance.

Proper choice	Actual choice by Dog 1	Dog 2	Dog 3	Dog 4
1 R	L	L	L	L
2 L	R	R	R	R
3 R	L	L	L	L
4 L	R	R	R	R
5 L	R	R	R	R
6 R	*	L	L	L
7 R	*	L	L	L
8 L	*	*	R	R
9 R	L	*	*	L
10 R	L	*	*	L
11 L	R	R	*	R
12 R	*	L	*	L
13 L	†	L	*	R
14 R		L	*	L
15 R		L	*	L

over which a buzzer had sounded. Dogs 1 and 3 gave up after making a few reactions of this kind; Dog 2, having given up the problem and acquired a new interest, relapsed into an ancient position-habit; while Dog 4, a very active half-grown puppy, maintained the old basis of choice throughout the series without reward. The following table shows the learning-record of Dogs 1, 2 and 4. Work with Dog 3 was discontinued after the fourth day, as at that time she attempted to escape through the door in the top of food compartment F′, near door X′, and became entangled with the gut cord by which door X′ is opened. In her struggles she succeeded in wrecking some apparatus and was so disturbed by the experience that for three successive days she refused to leave the home-box.

TABLE 9

Discrimination Between Buzzers, Their Positions Having Been Interchanged After Discrimination had Become Established

Day	Dog 1 % Accuracy	Dog 2 % Accuracy	Dog 4 % Accuracy
1	00	07	00
2	27	33	20
3	20	53	53
4	60	60	67
5	80	53	87
6	73	73	100
7	80	87	100
8	93	93	100
9	93	100	
10	100	100	
11	100	100	
12	100		

* Refused to work.
† Removed.

These results show nothing new except for the record of the first day. There may be some significance in a comparison of the records of Dogs 1 and 2 with that of Dog 4. Both the older dogs required a longer time to overcome the habit formed in problem 8 although the time for learning problem 8 was as short for Dog 2 as for the younger animal.

At this point the experiments had to be abandoned. I think it well, however, to outline the program which it was my intention to carry out had conditions permitted, as I believe it a safe approach to an investigation of pitch-discrimination. First, it was proposed to have the animals form the simple association required under the conditions of problem 8. Then, as a new problem, let the two buzzers be brought nearer and nearer together, until they shall be together on the table near the end of the introductory alley B. If the animals can still discriminate, the characteristic of localization is eliminated. Then let a stimulus fork be substituted for one buzzer, and if discrimination can be established and maintained between the two sounds, substitute the second stimulus-fork for the other buzzer. If the animals learn to discriminate between the two tones, variations of intensity and other controls can follow. Discrimination between noises lies well within the dog's capacity. It may yet be possible to obtain reliable evidence of discrimination on the basis of pitch-difference if the animal can be brought gradually to the point where it will be affected by the stimulus.

SUMMARY

The foregoing experiments have failed in showing to what extent the dog is sensitive to difference of pitch. They have not established that he is sensitive to pitch-difference at all. While they have not proved the contrary, they should have shown that we are not safe in accepting the assertion of Kalischer, Rothmann and Swift, that the dog has an exceedingly fine absolute pitch sensitivity. These experiments have shown that the evidence submitted to date does not warrant such a conclusion. They have also shown some of the difficulties in the way of making satisfactory tests on audition in animals, and should have demonstrated what is and what is not a reliable method of investigation in this field. It should also be

apparent that in using animals for operations to settle questions of cerebral localization, it is necessary to use methods of training far more complicated than many physiologists appear to appreciate. Certainly the remarkable variation in the findings of the investigators whose work has been considered in the foregoing pages, may be readily explained by reference to their methods of training.

APPENDIX I

On page 5 the reader was referred to a daily record made by one of my animals on pitch-discrimination which is reproduced below.

PROBLEM 6, DOG 3, DECEMBER 14, 1911

Trial	Proper choice	Actual choice
1	R	R
2	L	L
3	R	R
4	L	L
5	R	R
6	R	L
7	R	R
8	L	L
9	R	R
10	L	L
11	L	R
12	R	L
13	R	R
14	L	L
15	R	R

Correct reactions	12
Incorrect reactions	3
Total	15
Percentage of accuracy	80

Even superficial inspection will show that this animal was merely choosing the right and the left food-compartments alternately regardless of the stimulus given. She developed much more elaborate position-habits than this. This record is shown merely to call attention to the necessity of having several consecutive perfect daily records, with the order of presentation and secondary criteria under control, before assuming that the animal has learned to discriminate.

APPENDIX II

CONTROL EXPERIMENT ON DISCRIMINATION TO NOISE, AFTER LEARNING

In commenting on Rothmann's experiment (page 14) I emphasized the desirability of having all stimuli presented when the animal is occupying the same position relative to the food. The following record is more spectacular than some others which my animals have made, but I have noted numerous other instances which illustrate the point almost as well.

It has already been said that this dog had learned the problem. On the second day of her training she showed an invariable tendency to turn to the right food-compartment (F′). Each time on the second day and twice on the third day, immediately after the animal had made the wrong turn, the experimenter sounded the left buzzer the second time. When the control-test was made, eight days had elapsed since this had been done. This day's record, which follows, shows clearly how strong and persistent was the association.

PROBLEM 8, DOG 2, APRIL 22, 1912

Trial	Proper choice	Actual choice
1	R	R†
2	R	R†
3	L	L
4	R	R†
5	L	L
6	L	L
7	R	L*
8	L	L
9	R	L*
10	R	L*
11	R	L*
12	L	L
13	R	L*
14	R	R†
15	L	L

Correct reactions	10
Incorrect reactions	5
Total	15
Percentage of accuracy	66⅔

* Turned R; operator sounded (r) buzzer again; dog reversed choice.
† Stimulus presented only once.

II. COMPARISON OF LEARNING-TIME AND LEARNING-METHODS IN BLIND AND IN NORMAL DOGS

The behavior of the two temporarily blind dogs used in the experiments already described suggested another problem for which the animals could be used as material. It is evident that an animal trained to open the "puzzle-box" used by Thorndike [1] in work with cats and other animals and by Watson [2] and others in experiments on the white rat, must make a complicated and delicate adjustment in a minimal time. It should be interesting to ascertain to what extent the dog makes use of vision in making this adjustment; and to compare the methods employed by blind and by normal dogs in learning such problem. Further, these dogs had learned to behave in their ordinary environment practically as normal dogs, although none of their "spatial world" was "visual space." The "Molyneaux problem" is at once suggested, and it becomes of interest to note any changes in behavior concurring with the formation of a world of visual space.

Six food-boxes were constructed for this problem of 2″ x 2″ white pine framework covered with steel woven wire having a mesh about 1 cm. square. These boxes were each 30″ wide, 24″ long and 24″ high. A door 12″ x 12″ was cut in one of the 30″ x 24″ sides, and hung so as to be opened by a coiled spring when the latch was released. A sketch of one of these boxes is shown in Figure 4. The boxes will hereafter be referred to by number. The kind of latch and hanging of the door to each box is shown below.

Box No.	Kind of latch	Door opening
1	spoon-dip	inward
2	turn-button	outward
3	lift-bar	"
4	slide-bar	"
5	peg-in-hole	"
6	bobbin-string	inward

The program was to have each dog while yet blind learn a separate set of three boxes; then to note what disturbance, if any, followed total darkening of the room in which the work

[1] Thorndike, E. L. Animal Intelligence.
[2] Watson, John B. Animal Education.

had been learned; to note disturbances resulting from turning the boxes 90°, 180° and 270° respectively from the first position; and to make a retention-test after sixty days of rest. The dog's eyes were then to be opened. After recovery they were to be given their old problem boxes, for the purpose of noting any change in method that might take place; then each dog was to be made to learn the three problem-boxes which the other dog had learned while blind. The last part of this test was not carried out on Dogs 1 and 2, since neither showed enough evidence of vision to warrant the continuation of the experiment.

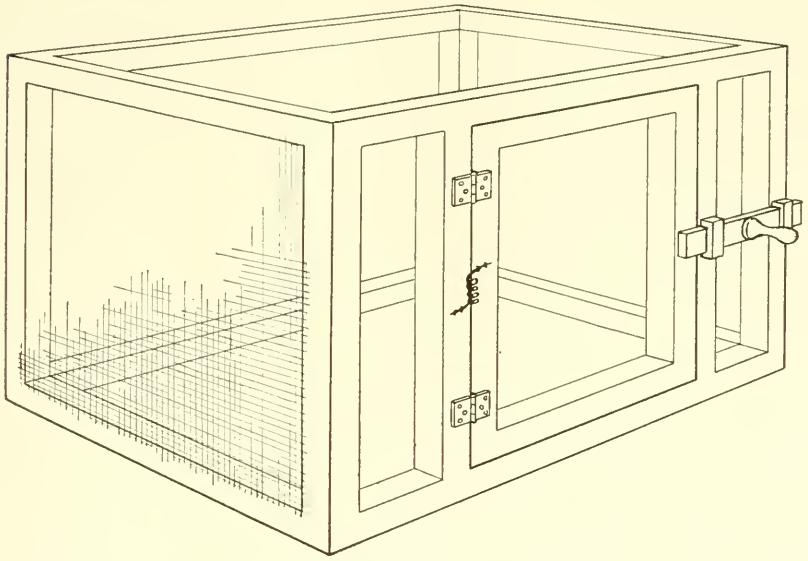

FIGURE 4—Sketch of problem-box 4

As to the animals used, Dogs 1 and 2, the temporarily blind females used in the work on audition, have already been described on p. 20. Dog 5 was a male of mixed breed—a cross between the bull-terrier and the black-and-tan—and was unusually quick, active and strong. He performed many tricks, among them jumping, catching a ball, etc., with great skill. Dogs 6, 7 and 8 were the offspring of Dog 5 and Dog 2. They were all males, littered June 1, 1911, in the psychological laboratory of the University of Chicago. Dog 6 was normal and cowardly.

Dogs 7 and 8 were rendered temporarily blind by the same method as that used on Dogs 1 and 2. Dr. Henry Dick, of the department of pathology of the University of Chicago, kindly performed the operation when the puppies were seven days old. Dog 7 was unusually large, but not clumsy. Despite his blindness he was able by reason of his fierceness and strength to maintain leadership of the eight dogs with which he was allowed to run. Dog 8 was small and decidedly of the terrier type. He was rather timid among the other dogs, and even in the laboratory his movements were deliberate. It should be said, however, that all the animals took readily to training of this

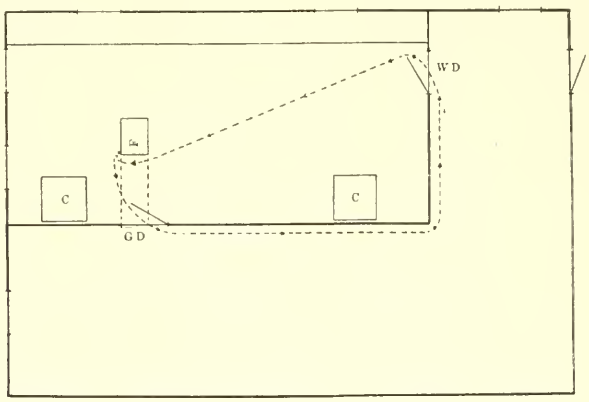

FIGURE 5—Floor plan of experimental-room. C, cages F, problem-box containing food; GD, glass exit door; WD, wooden entrance door; dotted line, habitual path of animal. Experimenter's station, outside room, by GD.

kind. They were fed once a day—during the daily test. Immediately after the series was completed for the day, each animal was allowed as much food as it would take. All the animals were in excellent, thrifty condition throughout the experimentation.

Dogs 1 and 2 were between four and five years old when they were introduced to this problem. Dog 5, a stray, was about one year of age. Dogs 6, 7 and 8 were five and one-half months old when work with them was begun. The experiments made on Dogs 1, 2 and 5 were between November 5, 1910 and March 20, 1911. Those made on Dogs 6, 7 and 8 were between November 15, 1911 and July 12, 1912.

The following method was employed in training: A room, the floor-plan of which is shown in Fig. 4, was partitioned off from a larger room, from which the observations were made The problem-box F was placed in the smaller room, sixteen feet from the wooden door W.D. in the diagram, and about four feet directly opposite the glass door G.D. where the operator stood. Food was placed in the problem-box, the door of which was left open for the first week. The animal which was left to run free in the larger room, was admitted through the wooden door W.D. to the smaller room, where it had already become accustomed to being fed. The wooden door opened by the operator pulling a rope running from it to the glass door, and was closed by a weight and pulley system after the animal had passed through. After the animal had obtained the food in the problem-box, it was released from the smaller room into the larger through the glass door G.D. Food was again placed in the problem-box and the animal readmitted through the wooden door as before. Every animal soon acquired a fixed path to and from the problem-box, which path is indicated by the dotted line and arrows in Fig. 4. By this method of training previous to the setting of the problem the animal's problem was simplified. There was no apparent disturbance from the operator's movements: as soon as the animal was released through the glass door G.D. it immediately ran to the wooden door and awaited readmission to the animal room; it was out of sight of the operator and wasted no time in trying to get food in other than the prescribed way. When the animal was presented to the problem-box the operator was out of the room, and in the actual experimentation, out of sight of the animal, as the larger room was darkened in the earlier stages of each new problem.

This general mode of procedure—the food-motive and the removal of the operator from the animal's immediate presence—has of course been used before by other experimenters and on other animals. It differs from the method of Thorndike. In his work the animals were confined in the puzzle box, it being assumed that the animal's desire to escape would be a satisfactory motive to bring about the opening of the box. The animals were dropped into the box from the top. Some of them being more nervous or timid than others, were more

greatly disturbed by this procedure; some apparently became frantic; the movements of others were inhibited probably from "fright." Still others, more quiet, became satisfied after making a few unsuccessful attempts to escape, and remained quiet for a long time. The defects n this method are obvious, and the advantages of the one adopted for this work will become more apparent when the animals' behavior is discussed.

After the animal had been fed from the open box for a week or more, one day's work was done with the door of the problem-box wedged open by only about two inches from the jamb. The dog had to force it open in some way in order to get the food. This was found necessary in the preliminary experiments because the animals would not try to open the box when it was latched. After sniffing and scratching lightly about the corner they would lie down for hours. This behavior lasted through three successive days and the experimenter believed that prolonging the animals' hunger could produce only harmful results. After one day of feeding from the partly closed box every animal quickly attacked the problem of opening the latched door.

Time was taken at each trial after the box was first latched. The record was of the number of seconds required for the animal to obtain the food after it had crossed the dotted line from the glass door to the corner of the problem-box. This point was chosen rather than the wooden door, because Dogs 1 and 2 were much heavier and slower of movement than any of the other dogs, and required one to two seconds more time to reach the problem-box from the wooden door, although their movements in opening the boxes were as swift and as accurate as were those of any of the other dogs. There was one disadvantage, however, in that it was difficult to tell within one-half second or so when the dog had crossed this line if the work was done in total darkness. Time was taken with an ordinary stop-watch. This records to 0.2 seconds. Fractional readings less than 0.5 seconds were disregarded; those greater than 0.5 second were counted one second. Some experimenters attempt to take time-readings in this way as closely as 0.2 seconds, and consider the fractions at full value in computing their results. In such work as this, however, the writer believes such a record shows an apparent accuracy which is not real. The experimenter's reaction-time will show considerable

variation; the ordinary cheap laboratory stop-watch may show a difference of 0.2 second in the time necessary to start and to stop it respectively; the instrument gets out of order easily, and the experimenter does not react to a movement of the animal which is absolutely constant for any two reactions. Further the writer does not believe a difference of 0.2 seconds to be significant in an animal's making so complicated a reaction as that of opening the problem-box.

In the experiments on Dogs 1, 2 and 5, twenty trials a day were allowed. Dogs 6, 7 and 8 were allowed only ten daily trials. It will be seen from the records that the dogs allowed only ten trials a day required fewer trials and in some cases even fewer days than did the animals given twenty trials under similar conditions. This may be explainable by the fact that after a certain amount of work has been done an animal may tend to become careless, through fatigue perhaps, and will relapse into errors, which, becoming habitual, persist for a considerable time. The tables show the distribution of errors made in this work by each dog, according to the place in the animal's daily series. It would be interesting to know just what is the optimal number of daily trials which should be allowed to a dog as compared with other animals in order to economize the learning process to the greatest degree.

TABLE 1

LEARNING RECORDS ON BOX 1

DOG 1 (BLIND ♀)

Problem 1

Day.....	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	Total
Trials																						errors
1	1	1	1	1	1	1	1	1			1			1		1						11
2	1	1	1	1	1	1		1		1		1	1	1		1						12
3	1	1	1	1	1			1	1		1	1	1	1								11
4	1	1	1	1	1			1			1	1	1	1								10
5	1	1	1	1	1	1		1		1	1	1	1	1								12
6	1	1	1	1	1	1		1		1		1	1	1								11
7	1	1	1	1	1	1	1	1				1	1									10
8	1	1	1	1	1	1					1		1	1		1		1				11
9	1	1	1	1	1	1					1	1	1	1								10
10	1	1	1	1	1	1	1			1	1	1	1		1							12
11	1	1	1	1	1	1		1			1	1	1	1								11
12	1	1	1	1	1	1						1	1	1	1	1						11
13	1	1	1	1	1	1			1	1	1	1	1		1							12
14	1	1	1	1		1		1	1	1				1	1	1						11
15	1	1	1	1	1	1	1			1		1				1						10
16	1	1	1	1			1	1		1	1	1	1									10
17	1	1	1	1		1	1		1	1			1				1					10
18	1	1	1		1	1	1		1	1			1		1							10
19	1	1	1			1	1	1		1		1	1		1							10
20	1	1	1		1	1		1		1		1	1		1		1					11
Total errors	20	20	20	17	16	17	8	12	5	12	10	15	17	11	7	6	2	1	0	0	0	216
% Error...	100	100	100	85	80	85	40	60	25	60	50	75	85	55	35	30	10	5	0	0	0	
% Accuracy	0	0	0	15	20	15	60	40	75	40	50	25	15	45	65	70	90	95	100	100	100	
Av. time...					5	0	10.4	2	7	2.9	2.1	1.5	1.5	1.9	1	9	1.5	1	3	1	2	1.1	1.1	1.0	1.0	1	0

TABLE 1—Continued

Learning Records on Box 1

Dog 5 (Normal ♂)

Problem 1

Days	1	2	3	4	5	6	7	8	9	10	11	12	Total errors
Trials													
1	1	1	1	1	1								5
2	1	1	1	1									4
3	1	1	1			1							4
4	1	1	1										3
5	1	1	1	1									4
6	1	1	1										3
7	1	1	1	1									4
8	1	1	1										3
9	1	1	1		1								4
10	1	1	1	1				1					5
11	1	1	1	1	1								5
12	1	1	1	1									4
13	1	1	1	1	1	1	1		1				8
14	1	1	1		1								4
15	1	1	1	1									4
16	1	1	1	1	1		1						6
17	1	1	1	1									4
18	1	1	1										3
19	1	1	1	1	1								5
20	1	1	1		1								4
Total errors	20	20	20	12	8	2	2	1	1	0	0	0	86
% Error...	100	100	100	60	40	10	10	5	5	0	0	0	
% Accuracy	0	0	0	40	60	90	90	95	95	100	100	100	
Av. time...	...	...	20.0	5.0	1.5	1.1	1.0	1.0	1.0	1.0	1.0	10.	

Dog 6 (Normal ♂)

Problem 2

Days	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	Total errors
Trials																							
1	1			1	1	1												1					5
2		1			1		1	1		1	1	1											7
3	1								1	1	1					1							5
4	1				1														1				3
5	1				1																		2
6	1				1	1						1	1				1						6
7	1	1			1		1									1		1	1				7
8	1	1	1	1			1	1		1		1	1				1						10
9		1		1		1		1	1			1		1									7
10	1		1					1															3
Total errors	8	4	2	3	6	3	3	4	2	3	2	4	2	1	2	0	3	2	1	0	0	0	55
% Error...	80	40	20	30	60	30	30	40	20	30	20	40	20	10	20	0	30	20	10	0	0	0	
% Accuracy	20	60	80	70	40	70	70	60	80	70	80	60	80	90	80	100	70	80	90	100	100	100	
Av. time...	3.3	1.8	1.2	1.4	1.9	1.2	1.2	1.0	1.3	1.0	1.0	1.1	1.0	1.0	1.2	1.0	1.0	1.1	1.0	1.0	1.0	1.0	

Dog 7 (Blind ♂)

Problem 2

Days	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	Total errors
Trials																										
1	1	1	1	1			1			1				1	1	1										9
2	1	1		1							1		1	1		1						1				8
3	1	1											1	1	1	1	1				1					8
4	1	1	1			1	1	1		1	1	1	1	1	1	1	1		1							15
5	1	1	1		1	1		1	1	1				1		1					1					11
6	1	1	1	1	1					1	1		1					1								9
7	1	1	1	1	1						1	1			1											8
8	1		1	1				1	1					1	1											7
9	1				1		1			1	1	1		1					1							8
10	1		1		1	1		1		1	1			1	1	1	1		1							12
Total errors	10	7	7	5	5	3	3	4	2	6	6	3	4	8	6	6	3	1	3	0	2	1	0	0	0	95
% Error...	100	70	70	50	50	30	30	40	20	60	60	30	40	80	60	60	30	10	30	0	20	10	0	0	0	
% Accuracy	0	30	30	50	50	70	70	60	80	40	40	70	60	20	40	40	70	90	70	100	80	90	100	100	100	
Av. time...	4.6	6.5	3.1	1.3	1.9	1.6	1.3	1.1	1.1	1.3	1.6	1.4	2.0	2.5	1.7	1.3	1.3	1.1	1.2	1.0	1.0	1.1	1.0	1.0	1.0	

TABLE 1—Continued

Learning Record on Box 1

Dog 8 (Blind ♂, After Operation)

Problem 5

Days.....	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	Total
Trials																		errors
1	1	1	1	1	1		1	1			1							8
2	1	1		1			1	1		1		1						7
3	1	1		1		1	1		1									6
4	1	1	1	1	1				1		1		1					8
5	1	1	1		1		1			1	1							7
6		1	1		1	1	1	1			1							7
7	1		1			1	1	1		1	1		1					8
8	1		1	1			1				1			1				6
9	1	1		1		1		1		1		1						7
10	1	1	1	1	1	1		1	1	1		1						10
Total errors	9	8	7	7	5	5	7	6	3	5	6	3	2	1	0	0	0	74
% Error...	90	80	70	70	50	50	70	60	30	50	60	30	20	10	0	0	0	
% Accuracy	10	20	30	30	50	50	30	40	70	50	40	70	80	90	100	100	100	
Av. time...	...	...	18.0	5 1	4 2	3 1	3.5	2.4	2.6	2.0	2.1	1.4	1.1	1.2	1.0	1.0	1.0	

TABLE 2

Learning Record on Box 2

Dog 2 (Blind ♀)

Problem 2

Days.....	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	Total
Trials																					errors
1	1	1	1	1	1	1	1	1	1	1		1		1	1						13
2	1	1	1	1	1	1	1	1	1	1		1									11
3	1	1	1	1		1		1	1	1		1				1					10
4	1	1	1	1	1	1	1	1	1						1						10
5	1	1	1	1	1	1	1		1	1				1	1						11
6	1	1	1	1	1			1	1	1		1		1							10
7	1	1	1	1	1	1	1	1	1	1					1						11
8	1	1	1	1	1	1	1	1	1	1				1	1						12
9	1	1	1	1	1	1		1	1	1		1		1							11
10	1	1	1	1	1	1	1	1	1	1											10
11	1	1	1	1	1	1	1	1													8
12	1	1	1	1	1	1									1						7
13	1	1	1	1	1	1	1					1			1		1				10
14	1	1	1	1	1	1			1	1	1			1	1						11
15	1	1	1	1	1	1	1	1	1					1	1						11
16	1	1	1	1	1	1	1	1	1					1	1	1					12
17	1	1	1	1	1	1	1	1	1	1		1		1	1						13
18	1	1	1	1	1	1	1	1	1					1	1						11
19	1	1	1	1	1	1	1	1	1				1	1	1						12
20	1	1	1	1	1	1	1	1	1		1	1		1	1						13
Total errors	20	20	20	20	19	19	15	16	17	11	2	8	1	12	14	2	1	0	0	0	217
% Error...	100	100	100	100	95	95	75	80	85	55	10	40	5	60	70	10	5	0	0	0	
% Accuracy	0	0	0	0	5	5	25	20	15	45	90	60	95	40	30	90	95	100	100	100	
Av. time...	3.3	2 6	3 4	3 2	3 1	2.7	2.1	2 3	1.2	1.8	1.3	1.6	1 6	1.6	2 3	1.6	1.1	1 2	1 1	1.0	

TABLE 2—Continued

LEARNING RECORDS ON BOX 2

DOG 5 (NORMAL ♂)

Problem 4

Days.....	1	2	3	4	5	6	7	8	9	10	11	12	13	14	Total
Trials															errors
1	1	1	1	1	1	1	1	1	1						9
2	1	1	1	1	1	1	1	1	1						9
3	1	1	1	1	1	1	1	1							8
4	1	1	1	1	1	1	1	1							8
5	1	1	1	1	1	1	1	1							8
6	1	1	1	1	1	1	1	1			1				9
7	1	1	1	1	1	1	1	1			1				9
8	1	1	1	1	1	1	1		1						8
9	1	1	1	1	1	1	1	1							8
10	1	1	1	1	1	1	1	1							8
11	1	1	1		1	1	1	1							7
12	1	1	1	1	1	1	1	1							8
13	1	1	1	1		1	1								6
14	1	1	1	1	1	1	1								7
15	1	1	1	1		1	1	1							7
16	1	1	1	1	1	1	1								7
17	1	1	1	1	1	1	1		1		1				9
18	1	1	1	1		1	1		1		1				8
19	1	1	1	1	1		1			1					7
20	1	1	1	1	1		1		1						7
Total errors	20	20	20	19	17	18	20	12	6	1	4	0	0	0	157
% Error...	100	100	100	95	85	90	100	60	30	5	20	0	0	0	
% Accuracy	0	0	0	5	15	10	0	40	70	95	80	100	100	100	
Av. time...	2.5	2.2	2.3	1.9	2.2	1.2	1.4	1.2	1.1	1.1	1.0	1.0	1.0	1.0	

DOG 6 (NORMAL ♂)

Problem 5

Days.....	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	Total
Trials																errors
1		1	1		1	1		1								5
2	1	1	1		1	1	1					1				7
3	1	1		1	1	1			1	1						7
4		1			1											2
5	1	1	1	1	1		1									6
6	1	1		1	1	1	1									6
7	1	1	1	1		1			1							6
8	1		1		1	1					1					5
9	1	1	1	1												4
10	1	1	1	1			1									5
Total errors	8	9	7	6	7	6	4	1	2	1	1	1	0	0	0	53
% Error...	80	90	70	60	70	60	40	10	20	10	10	10	0	0	0	
% Accuracy	20	10	30	40	30	40	60	90	80	90	90	90	100	100	100	
Av. time...	3.0	4.1	3.6	5.0	3.1	2.8	3.0	1.9	1.2	1.0	1.1	1.0	1.0	1.0	1.0	

DOG 7 (BLIND ♂, AFTER OPERATION)

Problem 5

Days.....	1	2	3	4	5	6	7	8	9	10	11	12	13	14	Total
Trials															errors
1	1	1	1	1	1	1		1							7
2	1	1	1	1	1		1			1	1				8
3	1	1		1			1		1	1					6
4	1	1	1				1								4
5	1	1	1	1	1	1	1		1						8
6	1	1		1	1		1								5
7	1		1	1	1		1								5
8	1		1	1	1	1		1							6
9	1	1	1		1			1							5
10	1	1			1	1									4
Total errors	10	8	7	7	8	4	6	3	2	2	1	0	0	0	58
% Error...	100	80	70	70	80	40	60	30	20	20	10	0	0	0	
% Accuracy	0	20	30	30	20	60	40	70	80	80	90	100	100	100	
Av. time...	22.0	9.1	4.7	3.9	3.1	2.2	2.6	2.0	1.7	1.4	1.5	1.0	1.1	1.0	

TABLE 2—Continued
LEARNING RECORDS ON BOX 2
DOG 8 (BLIND ♂)
Problem 2

Days.....	1	2	3	4	5	6	7	8	9	10	11	12	Total
Trials													errors
1	1	1	1	1	1		1						6
2	1		1	1									3
3	1	1	1	1									4
4	1	1	1	1									4
5	1	1		1	1	1							5
6	1								1				2
7	1		1	1									3
8		1	1	1									3
9				1		1							2
10		1	1	1									3
Total errors	7	6	7	9	2	2	1	0	1	0	0	0	35
% Error...	70	60	70	90	20	20	10	0	10	0	0	0	
% Accuracy	30	40	30	10	80	80	90	100	90	100	100	100	
Av. time...	72.0	2.1	6.3	8.0	2.2	1.8	1.9	1.4	1.1	1.0	1.0	1.0	

TABLE 3
LEARNING RECORDS ON BOX 3
DOG 1 (BLIND ♀)
Problem 3

Days.....	1	2	3	4	5	6	7	8	9	Total
Trials										errors
1	1	1				1				3
2	1	1	1	1	1					5
3	1	1	1							3
4	1		1							2
5	1		1		1					3
6		1				1				2
7				1	1					2
8		1								1
9		1								1
10	1									1
11	1									1
12	1									1
13	1									1
14	1									1
15	1	1								2
16	1									1
17	1									1
18	1									1
19	1									1
20	1	1	1							3
Total errors	16	8	5	2	3	2	0	0	0	36
% Error...	80	40	25	10	15	10	0	0	0	
% Accuracy	20	60	75	90	85	90	100	100	100	
Av. time...	—	7.5	1.2	1.4	1.1	1.1	1.1	1.0	1.0	

TABLE 3—Continued

LEARNING RECORDS ON BOX 3

DOG 5 (NORMAL ♂)

Problem 3

Days.....	1	2	3	4	5	6	Total
Trials							errors
1	1						1
2	1	1					2
3	1	1	1				3
4	1		1				2
5	1						1
6	1						1
7	1						1
8	1						1
9	1						1
10	1						1
11	1	1					2
12	1						1
13	1						1
14	1						1
15	1						1
16	1						1
17							0
18							0
19		1					1
20							0
Total errors	16	4	2	0	0	0	22
% Error...	80	20	10	0	0	0	
% Accuracy	20	80	90	100	100	100	
Av. time...	2.5	1.1	1.0	1.0	1.0	1.0	

DOG 6 (NORMAL ♂)

Problem 4

Days.....	1	2	3	4	5	6	7	8	9	10	11	12	13	Total
Trials														errors
1	1	1	1	1			1			1				6
2	1	1	1	1	1	1	1		1					8
3	1		1		1	1								4
4	1				1	1				1				4
5	1	1	1	1	1			1						6
6	1		1		1	1	1	1						6
7		1	1		1			1						4
8	1	1	1	1										4
9	1	1	1	1		1								5
10	1	1	1	1										4
Total errors	9	7	9	6	6	5	3	3	1	2	0	0	0	51
% Error...	90	70	90	60	60	50	30	30	10	20	0	0	0	
% Accuracy	10	30	10	40	40	50	70	70	90	80	100	100	100	
Av. time...	2.5	1.7	1.4	1.4	1.2	1.3	1.5	1.1	1.3	1.0	1.0	1.0	1.0	

DOG 7 (BLIND ♂, AFTER OPERATION)

Problem 4

Days.....	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	Total
Trials																	errors
1	1	1	1		1		1	1				1					7
2	1	1	1	1		1			1		1						7
3		1	1	1	1	1		1		1	1						8
4		1				1	1			1			1				5
5	1	1	1		1				1				1				6
6	1	1	1	1	1			1				1					7
7	1	1	1	1	1	1		1	1	1		1					10
8	1	1				1	1			1		1					6
9	1			1		1		1	1	1							6
10	1			1	1	1	1	1	1	1							8
Total errors	8	8	6	6	6	7	4	6	5	6	2	4	2	0	0	0	70
% Error....	80	80	60	60	60	70	40	60	50	60	20	40	20	0	0	0	
% Accuracy	20	20	40	40	40	30	60	40	50	40	80	60	80	100	100	100	
Av. time...	6.0	8.1	3.4	3 1	3.3	1 6	1 2	1 4	1 2	1 2	1 0	1.0	1.1	1.0	1.0	1.0	

TABLE 3—Continued

LEARNING RECORDS ON BOX 3

DOG 8 (BLIND ♂)

Problem 1

Days.....	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	Total
Trials																errors
1	1	1	1	1	1			1								6
2	1	1	1	1	1											5
3	1	1		1	1	1	1									6
4	1	1	1			1		1								5
5	1	1	1	1		1	1			1						7
6	1	1	1	1	1											5
7	1	1	1	1	1	1	1	1			1					9
8	1	1	1	1	1	1		1								7
9	1	1	1	1	1							1				6
10	1	1	1	1	1	1		1	1							8
Total errors	10	10	9	9	8	6	3	5	1	1	1	1	0	0	0	64
% Error...	100	100	90	90	80	60	30	50	10	10	10	10	0	0	0	
% Accuracy	0	0	10	10	20	40	70	50	90	90	90	90	100	100	100	
Av. time...	—	—	24.7	5.1	3.1	2.6	2.1	2.1	1.3	1.5	1.4	1.0	1.0	1.0	1.0	

TABLE 4

LEARNING RECORDS ON BOX 4

DOG 2 (BLIND ♀)

Problem 1

Days.....	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	Total
Trials																			errors
1	1	1	1	1	1	1	1	1	1					1	1				11
2	1	1	1	1	1	1	1		1	1									9
3	1	1	1	1	1	1	1	1			1				1				10
4	1	1	1		1	1	1	1	1	1									9
5	1	1	1	1	1	1	1		1				1						9
6	1	1	1	1	1	1	1	1	1	1									10
7	1	1		1	1	1	1	1	1										8
8	1	1	1	1	1	1			1	1				1					9
9	1	1		1	1	1			1										6
10	1	1		1	1	1	1	1	1										8
11	1	1	1	1	1	1	1	1	1			1							10
12	1	1	1	1	1	1			1										7
13	1	1	1	1	1	1	1	1	1		1								10
14	1	1	1		1	1	1	1	1	1		1							10
15	1	1		1	1	1		1	1			1							8
16	1	1		1	1	1			1	1									7
17	1	1			1	1		1	1		1								7
18	1	1			1	1	1	1	1	1									8
19	1	1	1	1	1	1	1		1										8
20	1	1	1	1	1	1	1	1	1	1									10
Total errors	20	20	13	16	20	20	14	13	19	8	3	3	1	2	2	0	0	0	174
% Error...	100	100	65	80	100	100	70	65	95	40	15	15	5	10	10	0	0	0	
% Accuracy	0	0	35	20	0	0	30	35	5	60	85	85	95	90	90	100	100	100	
Av. time...	—	—	12.0	3.5	3.3	3.2	1.8	2.1	2.1	2.4	1.1	1.3	1.7	1.3	1.3	1.1	1.0	1.0	

TABLE 4—Continued

Learning Records on Box 4

Dog 5 (Normal ♂)

Problem 2

Days.....	1	2	3	4	5	6	7	8	9	10	11	Total errors
Trials												
1	1	1	1	1	1	1	1	1				8
2	1	1	1	1	1	1						6
3	1	1		1								3
4	1		1	1		1						4
5	1	1		1		1						4
6	1	1		1								3
7	1	1		1	1							4
8	1	1	1	1								4
9	1	1	1	1		1						5
10	1	1		1								3
11	1	1	1	1		1						5
12	1	1	1	1	1							5
13	1	1	1	1	1							5
14	1	1	1	1		1						5
15	1	1	1	1	1	1						6
16	1	1	1	1	1	1		1				7
17	1	1	1	1	1							5
18	1	1										2
19	1	1	1		1							4
20	1	1	1									3
Total errors	20	19	14	17	9	9	1	2	0	0	0	91
% Error...	100	95	70	85	45	45	5	10	0	0	0	
% Accuracy	0	5	30	15	55	55	95	90	100	100	100	
Av. time...	—	—	—31.0	8.0	11.8	1.3	1.1	1.3	1.0	1.0	1.0	

Dog 6 (Normal ♂)

Problem 1

Days.....	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	Total errors
Trials																						
1	1	1	1			1	1	1	1	1	1	1	1	1		1						13
2	1	1		1	1		1	1	1	1	1		1		1		1					12
3	1	1	1	1	1	1	1	1		1												9
4	1	1		1	1		1	1	1		1	1	1	1								11
5	1	1			1	1	1	1				1		1			1					9
6	1	1	1		1	1	1	1		1	1		1	1		1		1				13
7	1	1	1	1	1	1	1	1	1	1		1	1	1			1					14
8	1	1		1	1	1	1	1		1			1	1		1	1					12
9	1	1	1		1	1	1	1	1	1	1			1			1					12
10	1	1	1	1		1	1			1				1								8
Total errors	10	10	6	6	8	8	10	9	5	8	5	4	6	8	1	3	5	1	0	0	0	113
% Error...	100	100	60	60	80	80	100	90	50	80	50	40	60	80	10	30	50	10	0	0	0	
% Accuracy	0	0	40	40	20	20	0	10	50	20	50	60	40	20	90	70	50	90	100	100	100	
Av. time...	—	—	2.9	2.3	2.6	5.0	1.7	3.3	2.2	2.6	1.9	1.5	2.5	2.0	1.3	1.3	1.4	1.1	1.0	1.0	1.0	

Dog 7 (Blind ♂)

Problem 1

Days.....	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	Tota errors
Trials																		
1	1	1	1	1	1	1	1	1		1	1	1						11
2	1	1	1	1	1	1	1		1	1			1	1				11
3	1	1	1	1	1		1		1				1					8
4	1	1	1	1	1		1		1	1								8
5	1	1	1	1	1		1		1									7
6	1	1	1	1	1				1			1						7
7	1	1		1	1	1	1		1									7
8	1	1	1	1	1		1		1		1							8
9	1	1	1	1	1		1	1				1						8
10	1	1	1			1	1	1			1							7
Total errors	10	10	9	9	9	4	9	3	7	3	3	3	2	1	0	0	0	82
% Error...	100	100	90	90	90	40	90	30	70	30	30	30	20	10	0	0	0	
% Accuracy	0	0	10	10	10	60	10	70	30	70	70	70	80	90	100	100	100	
Av. time...	—	—	—22	8.3	4.3	8.2	7.4	4.2	6.2	3.1	9.1	2.1	7.1	3.1	1.1	1.0	1.0	

TABLE 4—Continued

Learning Records on Box 4

Dog 8 (Blind ♂, After Operation)

Problem 4

Days.....	1	2	3	4	5	6	7	8	9	10	11	12	13	14	Total
Trials															errors
1	1	1	1			1			1						5
2	1	1	1	1	1	1		1							7
3	1		1	1	1		1				1				6
4	1	1	1	1			1			1					6
5			1	1	1			1							4
6		1		1		1									3
7	1	1		1											3
8		1	1			1									3
9	1	1	1			1	1								5
10	1	1	1												3
Total errors	7	8	8	6	3	5	3	2	1	1	1	0	0	0	45
% Error...	70	80	80	60	30	50	30	20	10	10	10	0	0	0	
% Accuracy	30	20	20	40	70	50	70	80	90	90	90	100	100	100	
Av. time...	4.0	3.4	2.8	2.2	2.6	1.9	1.4	1.6	1.1	1.3	1.0	1.0	1.0	1.0	

TABLE 5

Learning Records on Box 5

Dog 1 (Blind ♀)

Problem 2

Days.....	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	Total
Trials																				errors
1	1	1	1	1	1	1	1		1	1		1								10
2	1	1	1	1			1				1									6
3	1	1	1	1	1	1	1			1										8
4	1	1	1	1	1	1	1	1	1		1									10
5	1	1	1	1	1			1	1		1									8
6	1	1	1		1										1					5
7	1	1	1		1															4
8	1	1		1	1			1	1			1								7
9	1	1	1							1										4
10	1	1	1	1						1										5
11	1	1	1				1		1				1							6
12	1	1	1	1		1	1	1	1	1	1				1					11
13	1	1	1					1	1	1				1		1				8
14	1	1	1	1	1			1	1	1	1									9
15	1	1		1							1		1							5
16	1	1	1	1	1		1	1												7
17	1	1	1		1	1		1	1	1		1								9
18	1	1	1	1		1			1	1										7
19	1	1	1	1	1		1	1	1	1	1	1								11
20	1	1	1	1	1	1	1	1	1	1										10
Total errors	20	20	18	14	12	7	9	10	12	11	7	4	2	1	2	1	0	0	0	150
% Error...	100	100	90	70	60	35	45	50	60	55	35	20	10	5	10	5	0	0	0	
% Accuracy	0	0	10	30	40	65	55	50	40	45	65	80	90	95	90	95	100	100	100	
Av. time...	...	...	3.8	2.5	2.4	1.5	1.7	3.3	2 1	1.9	1.6	1.5	1.2	1.1	1.1	1.1	1.0	1.0	1.0	

TABLE 5—Continued

LEARNING RECORDS ON BOX 5

DOG 5 (NORMAL ♂)

Problem 5

Days..... / Trials	1	2	3	4	5	6	7	8	9	10	Total errors
1	1	1	1	1							4
2	1		1				1				3
3	1	1	1	1							4
4	1	1	1								3
5	1										1
6	1	1	1	1							4
7	1	1	1		1						4
8	1	1		1							3
9	1	1									2
10	1	1	1								3
11	1	1	1								3
12	1	1	1								3
13	1	1	1		1	1					5
14	1	1									2
15	1										1
16	1		1				1				3
17		1	1								2
18		1									1
19		1		1							2
20	1	1									2
Total errors	17	16	12	5	2	1	2	0	0	0	55
% Error...	85	80	60	25	10	5	10	0	0	0	
% Accuracy	15	20	40	75	90	95	90	100	100	100	
Av. time..	—	3.3	2.5	2.0	1.2	1.2	1.0	1.0	1.0	1.0	

DOG 6 (NORMAL ♂)

Problem 6

Days..... / Trials	1	2	3	4	5	6	7	8	9	10	Total errors
1	1	1	1				1				4
2	1	1		1							3
3	1			1							2
4			1								1
5	1	1					1				3
6	1	1			1						3
7		1									1
8	1	1	1								3
9	1	1	1								3
10	1										1
Total errors	8	7	4	2	1	0	2	0	0	0	24
% Error...	80	70	40	20	10	0	20	0	0	0	
% Accuracy	20	30	60	80	90	100	80	100	100	100	
Av. time...	3.8	2.9	2.7	1.6	1.1	1.0	1.3	1.0	1.0	1.0	

DOG 7 (BLIND ♂, AFTER OPERATION)

Problem 6

Days..... / Trials	1	2	3	4	5	6	7	8	9	10	11	Total errors
1	1	1	1	1			1					5
2	1	1	1	1		1		1				6
3	1	1	1									3
4	1	1		1								3
5	1				1							2
6	1		1									2
7	1	1	1		1	1						5
8	1	1	1									3
9	1	1	1									3
10	1	1		1		1						4
Total errors	10	8	7	4	2	3	1	1	0	0	0	36
% Error...	100	80	70	40	20	30	10	10	0	0	0	
% Accuracy	0	20	30	60	80	70	90	90	100	100	100	
Av. time...	...	...	7.0	2.6	2.5	1.9	2.2	1.6	1.4	1.3	1.0	

TABLE 5—Continued

Learning Records on Box 5

Dog 8 (Blind ♂)

Problem 3

Days.....	1	2	3	4	5	6	7	8	9	10	11	12	13	Total
Trials														errors
1	1	1	1	1		1		1						6
2	1	1	1	1					1					5
3	1	1	1	1	1									5
4	1	1	1		1	1	1							6
5	1	1	1		1	1	1			1				7
6	1	1	1			1								4
7	1	1	1	1				1						5
8	1	1	1	1				1						5
9	1	1		1	1	1		1						6
10	1	1	1		1	1	1							6
Total errors	10	10	9	6	5	6	3	4	1	1	0	0	0	55
% Error...	100	100	90	60	50	60	30	40	10	10	0	0	0	
% Accuracy	0	0	10	40	50	40	70	60	90	90	100	100	100	
Av. time...	—	—	5.8	4.1	2.9	2.5	2.3	2.7	2.0	1.8	1.9	1.8	1.5	

TABLE 6

Learning Records on Box 6

Dog 2 (Blind ♀)

Problem 3

Days.....	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	Total
Trials																	errors
1	1	1	1	1		1	1	1	1	1	1	1					11
2	1	1	1	1	1	1			1	1	1						9
3	1	1		1	1	1	1	1	1								8
4	1	1	1	1	1	1	1		1				1				9
5	1	1	1	1	1	1	1	1	1		1						10
6	1	1		1		1	1	1	1	1							8
7		1	1	1		1	1	1			1						7
8	1	1	1	1	1	1	1	1		1							9
9	1	1		1	1			1		1							6
10		1	1	1	1		1	1		1							7
11		1	1	1	1	1	1		1	1	1						9
12		1	1	1	1	1				1	1						7
13	1	1	1		1	1	1		1	1	1	1					10
14	1	1	1	1	1	1	1			1		1					9
15	1	1	1	1	1	1	1			1			1				9
16	1	1		1	1		1			1							6
17	1	1	1	1	1	1			1	1							8
18	1	1	1	1	1	1	1		1	1							9
19	1	1	1	1			1	1	1	1							8
20	1	1	1	1	1	1	1		1	1							9
Total errors	16	20	16	19	16	16	16	9	12	16	7	3	2	0	0	0	168
% Error...	80	100	80	95	80	80	80	45	60	80	35	15	10	0	0	0	
% Accuracy	20	0	20	5	20	20	20	55	40	20	65	85	90	100	100	100	
Av. time...	...	8.3	8.3	4.8	4.3	2.9	2.6	2.7	1.6	2.1	1.9	1.6	1.3	1.3	1.0	1.0	

TABLE 6—Continued

LEARNING RECORDS ON BOX 6

DOG 5 (NORMAL ♂)

Problem 6

Days..... / Trials	1	2	3	4	5	6	7	8	9	10	Total errors
1	1		1								2
2	1	1			1						3
3	1					1					2
4	1	1		1							3
5	1		1								2
6	1	1	1		1		1				5
7	1	1				1					3
8		1		1							2
9		1	1		1						3
10	1	1		1							3
11		1									1
12		1	1	1	1		1				5
13											0
14											0
15	1	1		1		1					4
16	1	1		1		1					4
17		1	1								2
18		1	1		1						3
19		1	1	1		1					4
20		1	1		1						3
Total errors	10	15	9	7	6	5	2	0	0	0	54
% Error...	50	75	45	35	30	25	10	0	0	0	
% Accuracy	50	25	55	65	70	75	90	100	100	100	
Av. time...	—	2.4	1.8	1.6	1.3	1.2	1.2	1.0	1.0	1.0	

DOG 6 (NORMAL ♂)

Problem 3

Days..... / Trials	1	2	3	4	5	6	7	8	9	10	11	Total errors
1	1	1	1	1	1							5
2	1	1	1	1	1		1					6
3	1	1		1								3
4	1	1		1	1	1						5
5	1	1	1		1		1					5
6	1	1	1		1							4
7	1	1	1									3
8	1	1	1		1		1	1				6
9	1	1		1		1						4
10	1	1	1	1								4
Total errors	10	10	7	6	6	2	3	1	0	0	0	45
% Error...	100	100	70	60	60	20	30	10	0	0	0	
% Accuracy	0	0	30	40	40	80	70	90	100	100	100	
Av. time...	4.5	1.9	2.2	1.7	2.0	1.1	1.3	1.0	1.5	1.0	1.0	

DOG 7 (BLIND ♂)

Problem 3

Days..... / Trials	1	2	3	4	5	6	7	8	9	10	11	Total errors
1	1		1	1		1						4
2	1		1	1			1					4
3		1	1	1								3
4	1	1	1	1	1		1					6
5			1		1	1						3
6	1	1	1	1	1			1				6
7	1		1	1	1							4
8		1	1		1							3
9	1			1								2
10	1	1		1	1							4
Total errors	7	5	8	8	6	2	2	1	0	0	0	39
% Error...	70	50	80	80	60	20	20	10	0	0	0	
% Accuracy	30	50	20	20	40	80	80	90	100	100	100	
Av. time...	6.5	1.4	5.1	3.6	1.5	1.2	1.1	1.0	1 0	1.0	1.0	

TABLE 6—Continued

LEARNING RECORDS ON BOX 6

DOG 8 (BLIND ♂, AFTER OPERATION)

Problem 6

Days.....	1	2	3	4	5	6	7	8	9	10	11	12	Total
Trials													errors
1	1	1	1	1	1	1							6
2	1	1		1			1						4
3	1	1		1		1			1				5
4	1	1	1	1	1								5
5	1	1	1	1				1					5
6	1	1	1	1	1								5
7	1	1	1	1	1		1						6
8	1	1	1	1			1						5
9		1			1								2
10	1	1	1		1								4
Total errors	9	10	7	8	6	2	3	1	1	0	0	0	47
% Error...	90	100	70	80	60	20	30	10	10	0	0	0	
% Accuracy	10	0	30	20	40	80	70	90	90	100	100	100	
Av. time...	—	—	3.2	3.1	2.1	2.3	1.7	1.9	1.4	1.2	1.0	1.1	

Dogs 1 and 2 were operated on, 25 March, 1911, by Dr. Walter E. Dandy, of the Johns Hopkins Hospital. The operation was quite successful and healing followed without a trace of infection, but the corneas were found in bad condition. About three weeks after the operation Dog 2 received a scratch on the right cornea during a fight. This wound became infected and an abscess near the iris followed. So far as gross behavior indicated the animal was blind. Some tests were made on Dog 1 to discover whether the eyes were functioning. (Her corneas were not as clouded in appearance as were those of Dog 2; the pupils reacted well to light and she seemed in better general condition.)

Following the operation the dogs had been kept in a dark-room for ten days, until the wounds had entirely healed. Dog 1 was then taken into the laboratory and subjected to a few rough tests of the presence of vision. The experimenter stood at one end of a room forty feet long, holding food in his hand. The animal was held by another person at the opposite end of the room. When the experimenter called, the animal was released and came quickly to the experimenter for food. This was repeated four or five times. Then a white pine board, two feet wide and eight feet long was set on edge and supported by props placed against the side away from the animal. When the dog was called she ran directly into the board. The position of the latter was changed four or five times but with no effect. A rope was then stretched across the room. The animal tripped over it each time, but after the fourth or fifth trial began

to hesitate when she approached the region where she had tripped before. Twice she jumped when she passed near where the rope had been stretched at the previous trial. The experiment was not continued further at that time, as prolongation of the tests seemed to be mere cruelty.

The same test was now given to Dog 5, the normal male. He always ran around or jumped over the board. The first trial given after the rope was stretched he tripped over the rope, turning a complete half-somersault. In subsequent trials he always jumped. The rope was held in the same place for three successive trials and then removed. In the next three successive trials when the animal reached the place where the rope had been stretched he jumped as he had jumped when the rope was there. The rope was then stretched again, two feet nearer the animal. This time the dog tripped by jumping too late—i.e., he jumped at about the same place as in the previous trials. In subsequent trials he always adjusted himself correctly.

The same tests were repeated on Dog 1 a week later. She avoided the board from the first, by means of what sense-avenues we of course do not know. She never succeeded in adjusting herself to the rope, however, always jumping too late or too soon.

These crude tests are further indication of the slight use of vision by the normal dog, and made it seem improbable that Dogs 1 and 2 would change their mode of attack on the problem-boxes because of the little vision they seemed to have. Accordingly it was decided to discontinue this work on them. It seemed questionable, however, whether better results might not be obtained by using animals which had not been kept in the blind state as long as had these. This was the reason for having the same operation performed on Dogs 7 and 8. I wish to say in this connection that our experience has not shown this operation useful for this work. It is not in itself cruel, and the only serious inconvenience which the blind puppies showed was their inability to compete with the normal puppies during the nursing period. The normal puppies would follow the mother into places in the animal's yard where the blind puppies would not go, and would often nurse when the blind puppies were not even present. After weaning, Dog 7 and

another blind litter brother not used in these experiments actually outgrew all the other members of the litter of seven. Dog 8, which was smaller at birth thrived well, while another blind male of the same litter, which was rather weakly from the beginning, died at four months of indigestion. There is no reason why a puppy which has undergone this operation should not live as thriftily and happily as a normal puppy if given proper care. But in clipping away enough of the edges of the eyelids to insure the formation of strong scar tissue some small glands are necessarily destroyed. While the dog is kept blinded, too, the small drain which is left at the corner of the eye is likely to become clogged and a slight infection may develop sufficiently to injure the cornea. The apparatus of accommodation may atrophy for want of use. A histological examination of the optic tracts of Dogs 1 and 2 will be made at some time in the future, in order to determine whether there was deterioration there.

Dogs 7 and 8 were operated on, 30 May, 1912, by Dr. Conrad Jacobson of the Johns Hopkins Hospital. Recovery was uneventful. The right cornea of Dog 8 was found considerably clouded, but cleared up to some degree in about two weeks after the operation. Dog 7 was apparently in good condition, although it was difficult to detect any change in his behavior. Substantially the same test was performed on him and Dog 8 as was made on Dog 1. There were no other persons present, so the obstruction, a board two feet wide and seven feet long, covered with white cheesecloth was placed directly in his habitual path from one room to another. Its position was changed after each trial. The door was opened by a weight-and-pulley system, and the operator released it just before calling the dog. Dog 8 collided once with the board, avoided it the next two consecutive times, collided the third and afterwards came into the room crouching and keeping to the wall. Dog 7 never bumped into the board. These tests were made fourteen days after the operation. Dog 8 avoided the board successfully in each of ten trials on the twenty-first day after the operation. In their other behavior absolutely no other change could be noted. Both of them in the blind state had found their way about the laboratory and yard. From about a month before the operation the animals were kept in a building at Home-

wood and at times were given the freedom of a lawn covering several acres for an hour or more at a time. On 16 April, 1912 Dogs 7 and 8 while blind escaped with Dog 6, a normal male of the same litter and chased a half-grown cat together. The latter took refuge in a corner of a porch and was caught and killed by Dog 7. It was not possible in the time left to the experimenter to make a test of the vision of Dog 7, the animal which after the operation seemed most nearly normal. It cannot be ventured how much or how little vision either of these animals had. Inasmuch as their records while blind compare very favorably with that of the normal dogs I have only negative and nconclusive evidence to offer on the question which was the primary object of this investigation. What data I have gathered seem to show only this positive fact: that the dog is capable of learning to make complicated adjustments and of performing a surprising number of "instinctive" reactions perfectly without the aid of vision. This fact remains established no matter what later tests may show regarding the dog's ability to make visual discriminations.

RETENTION

Sixty days after the third problem had been learned by each dog, a test was made of the retention of the learning of the first three boxes. The results are not shown because there was absolutely no significant variation among the records of the different animals. None showed a loss of accuracy of over 10% on the first day's work in the retention test, and in only one case was over three days' work necessary to regain the loss. This exception was Dog 2 on box 1. In learning the problem the animal had used first her paw, then her nose, to depress the latch. She settled on the nose-method. In the retention-tests the paw-method reappeared and persisted for six days before the nose-method was completely re-established. The time required for opening the box by either method was about the same—not over one second.

CONTROL TESTS

Two control tests were made on Dogs 1, 2 and 5. The first was for the purpose of observing what disturbance resulted from requiring the dogs to perform their work in total darkness. Two

boxes, which the animals had learned and were then opening in one second or less, were provided with contacts of brass. When the door of the box was closed, a circuit was formed, the current running from the positive pole of a dry battery through the magnet of a Schallhammer in the experimentor's room, to the metal contacts on the door and jamb of the box, to the negative pole of the battery. When the latch of the door was released this circuit was broken and the hammer being drawn from the magnet by a spring gave a signal-click. The animal-room was darkened by heavy black tar-paper nailed over the windows and openings about the door. The tests were made at night and all lights in the animal-room and the room adjoining were put out before the animal was admitted to the experiment-room. Time was taken from the animal's passing between the glass door and the food-box, and the click of the Schallhammer when the box was opened. This is accurate to within one-half second. The blind dogs were slightly disturbed, as is shown by the fact that the time of their first ten reactions varied between four and seven seconds; the next ten averaged about two. Dog 5, normal, was much more disturbed. His first reaction under the changed conditions required thirteen seconds and there were many useless movements. Several times in the first six trials he climbed on top of the box and scratched there. The reactions after the sixth were more definite, and after the twelfth there were no more errors, and the time for each thereafter averaged 1.3 seconds for thirty trials. These results indicate that the light had some stimulating effect, on both the blind and the normal dogs.

The second control was a test on Dogs 1, 2 and 5, of disturbance when a box previously learned by the dog was oriented in another direction. As the boxes stood when the animals were learning them, the latch was always at the northwest corner; when the box was turned 90°, then the latch was on the northeast corner, etc. The following table summarizes the disturbance which follows:

TABLE 7

DISTURBANCE FOLLOWING ROTATION OF PROBLEM BOX ALREADY LEARNED

Dog 1. Blind ♀. Box 4

Position of latch-corner of box	Day	No. trials	% Accuracy	Av. time seconds
N.W.	1	10	100	2.2
N. E.	1	10	40	—
"	2	20	80	2.6
"	3	10	100	3.2
S. E.	3	10	20	3.4
"	4	20	60	3.0
"	5	20	100	2.9
"	6	10	100	3.0
S. W.	6	10	00	5.8
"	7	20	40	3.6
"	8	20	80	7.3
"	9	20	90	4.2
"	10	20	100	3.0

Dog 2. Blind ♀. Box 1

Position of latch-corner of box	Day	No. trials	% Accuracy	Av. time seconds
N. W.	1	10	100	1.1
N. E.	1	10	00	11.0
"	2	20	70	4.1
"	3	20	95	2.0
S. E.	4	20	00	6.9
"	5	20	90	3.5
"	6	20	95	2.5
"	7	10	100	2.2
S. W.	7	10	50	2.0
"	8	20	70	3.0
"	9	20	100	2.0

Dog 5. Normal ♂. Box 1

Position of latch-corner of box	Day	No. trials	% Accuracy	Av. time seconds
N. W.	1	10	100	1.0
N. E.	1	10	60	9.1
"	2	20	90	1.0
"	3	10	100	1.0
S. E.	3	10	90	2.0
"	4	20	95	2.5
"	5	10	100	2.0
S. W.	5	10	50	2.6
"	6	20	80	2.0
"	7	20	100	2.0

The animals' behavior in this new situation is interesting. The turning of the box 90° made the problem practically a new one for each dog. Every animal attacked the box at the northwest corner, as if the latch were still there. After the usual methods of "nosing," scratching and biting had failed, the animals resorted to violent random movements, scratching

over the north side of the box, walking around it, barking all the while. Dog 1 required several hours for opening the box the first time. A period of work, which never lasted over ninety seconds, and which averaged near fifteen, would be followed by a prolonged rest. This is characteristic of all my animals' reaction to a new box. After an animal had opened the box once or twice in the new position, the time necessary for opening was quickly shortened and the principal errors made thereafter were those of hesitation or attack at the northwest corner.

Dog 5 surprised the members of the laboratory who ventured to predict his behavior when the box was first turned 90°. The writer expected him to "hunt for the latch." He showed fully as much disturbance as either blind dog. When first admitted to the experiment room he went directly to the northwest corner, scratched violently at the wire over the whole north side, dragged the box, which was heavily weighted, some five inches with his teeth, climbed on top of the box, then lay down to rest after eighty-five seconds of work. He opened the box at the second attack, after accidentally touching the latch with his head. I wish to stress one feature of his behavior. Several times during his periods of effort his head and nose came within an inch of the projecting (lift) latch bar. His actions were not affected in the least by proximity to the latch until he actually *touched* the latch. Then, in less time than is necessary for working a stop-watch lever, he or ented himself to the latch and opened the door by a single movement.

Much less disturbance was shown when the box was turned another 90°. The animals went to the southeast corner after only a few seconds of effort at the northeast corner. At this point the first difference in *method* appeared. Dogs 2 and 3 never changed their customary pathway. They continued to pass to the southeast corner by way of the northwest and northeast corners. Dog 5 went directly to the southeast corner after the seventh trial.

When the box was turned another 90° farther, making the latch corner southwest, Dog 5 went first to the southeast corner and paused long enough to give only one scratch, then hastened to the southwest corner, to which in subsequent trials he went directly. His errors were those of passing too far to the north before orienting himself to the latch, and attacking the latch

too far from the free end to open the box with one effort. The blind dogs at the first trial passed around the box, stopping to scratch at the southeast end, and then trying the southeast and northeast corners before attacking the latch at the southwest. Both learned to stop at the southwest corner during the first series of ten trials: Dog 1 at the third and Dog 2 at the eighth trial.

SUMMARY

The above experiments have shown that it is impracticable to attack the "Molyneux problem" by using dogs rendered temporarily blind by this operation.

It is evident from the records that vision is not necessary to enable the dog to make quite complicated adjustments. The behavior of my animals, particularly that of Dog 5 in the control tests indicates that the dog may make little use of vision. His reaction to the rope and to the disoriented box strengthens the belief that the normal as well as the blind dogs, in ordinary situations rely greatly on kinaesthetic and muscular sense-processes in making their adjustments. This accords with the data accumulated by Watson, Richardson and Vincent in the rat.

The rate and methods of learning in blind and normal dogs shows surprisingly little difference.

Dogs given only ten trials a day required fewer trials for learning, on the average, than those given twenty trials. This fact suggests the question, what is the optimal number of daily trials to educe perfect habits with the least effort?

Behavior Monographs

Volume 1

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BEHAVIOR MONOGRAPHS

Volume 2, Number 4, 1914 Serial Number 9

Edited by JOHN B. WATSON
The Johns Hopkins University

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GARDNER CHENEY BASSET

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at Cambridge, Boston, Mass.
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CONTENTS

		PAGE
I.	Introduction	1
II.	Methods	2
III.	Experiment 1: the Maze	5
IV.	Experiment 2: the Preliminary Inclined Plane	25
V.	Experiment 3: the Inclined Plane	30
VI.	Summary and Conclusions	43

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Before entering upon the body of this presentation I desire to express my obligations to those without whose co-operation my experiments would have been of less value.

Above all am I indebted to Professor John B. Watson, Director of the Johns Hopkins Psychological Laboratory, who has kept himself informed of the progress of my experimentation and who has been ready at all times with helpful suggestions and encouragement.

To Dr. Henry H. Donaldson I owe much: for suggesting the experiment; for placing the facilities and materials of the Wistar Institute at my disposal; for much helpful advice as to the evaluation of my results.

To Dr. Shinkishi Hatai for preparing the anatomical data referring to the rats used in my experiments.

To Dr. Helen D. King for keeping her sequences of inbreeding moving so perfectly that it was possible at any time to procure inbred rats of the desired age.

GARDNER CHENEY BASSET.

I. INTRODUCTION

A few years ago experimental inbreeding of the albino rat, *Mus norvegicus albinus*, was started at the Wistar Institute of Anatomy and Biology in order to determine the anatomical consequences of such procedure upon successive generations of progeny. Among other results obtained was a distinct and progressive decrease in actual and relative brain weight (relative, that is, in reference to body length) for four generations of close inbreeding. At the end of the fourth generation the rats seemed lacking in vitality and, for this reason, were subjected to a change in food. From this period until the end of the tenth generation (the extent of inbreeding at the time this paper was prepared) the relative brain weight remained, on the average, constant at six and one-half per cent less than that of the normal control rats.

When, early in October, 1911, Dr. Donaldson suggested to Professor Watson that the decrease in brain weight might be accompanied by a similar decrease in ability to form habits a new line of investigation in comparative psychology was opened up. The problem was offered to the writer and gladly accepted.

It is no part of the purpose of this paper to raise the question as to whether inbreeding, *per se*, results deleteriously upon the progeny. In this, as in all disputed questions, it is unsafe to be arbitrary, and authoritative testimony must await the results of further investigations. We know, upon the authority of historians, that the Incas of Peru for many generations married their sisters and were physically and mentally superior to their subjects. Breeders of domestic animals frequently resort to inbreeding in order to perfect desirable qualities in the strain. It may be, as many claim, that inbreeding results deleteriously only in cases where an hereditary taint, occurring in the common ancestor, is strengthened in the progeny of a consanguineous union. Of the rats used in the experiments hereinafter described, it is not postulated that the lesser ability to form habits is necessarily due either to inbreeding or to the environmental factor of

insufficient nourishment during the first four generations; but, the rats used for purposes of inbreeding produced a strain having a lesser relative brain weight on the average. This strain of rats I shall hereafter, for convenience, refer to as the Inbred Strain. The object, then, of the following experiments is to compare the habit-forming ability of the inbred strain *with lesser brain weights*, with the ability of a normal control series.

Owing to the fact that experimental work on the brain weight problem has not before been attempted there is no history and little literature to be presented. Donaldson[1] reproduces tables from Manouvrier[2] showing the brain weights of eminent men to be, on the average, greater than those of average Parisians. It is not necessarily true that the specific individual with greater brain weight is more intelligent or will contribute more to the world's arts and sciences than the specific individual of lesser brain weight; but, if the conclusions of Manouvrier are to be believed, individuals of brain weight above the average are more liable to be of superior intelligence and to do the world greater service.

The results of the experiments described in this paper agree closely with Manouvrier's conclusions. Tables of distribution of brain weights of the inbred strain and normal control series overlap; but the normal series, having a greater brain weight average, show greater ability in habit formation.

All the experiments here described were carried out at the Psychological Laboratory of the Johns Hopkins University.

II. METHODS

All the inbred rats used in this investigation were bred at the Wistar Institute of Anatomy and Biology by Dr. Helen D. King. Two strains were used, referred to in this paper as strains A and B. The original parents of each strain were taken at random, a male and female from each of two unrelated litters. The A male was mated to his sister, A female, and the B male to his sister, B female. Their respective litters constituted generation 1A and 1B. From this point inbreeding was carried on by selecting from the litter the healthiest appearing

[1] Donaldson: The Growth of the Brain. London and New York, 1909, pp. 128 ff.

[2] Manouvrier, Sur l'interprétation de la quantité dans l'encéphale, etc., Paris, 1885.

rats and mating brother to sister within the same litter, this constituting the closest possible inbreeding. At about thirty days of age the young rats were taken from their mother, and those to be used by the writer were shipped to the Johns Hopkins University. There were no fatalities in transit and all arrived apparently in good condition. The system of numbering individual rats for identification was as follows: the first number referred to the generation of inbreeding, the letter (A or B) to the strain, and the last number was that applied to the individual. For example, 7A90♀ may be analyzed as follows: 7th generation inbred, A strain, individual 90, female. Each rat had one or both ears punched or clipped to agree with the individual number, a system in use by Professors Castle and Yerkes at the Harvard laboratories.

It seemed advisable to secure normal control mating strains from different laboratories in order to avoid any possibility of inbreeding. In addition to our own Hopkins stock there were obtained rats from the Wistar Institute, Columbia University, animal dealers in Baltimore, and from Dr. Herbert M. Evans of the Johns Hopkins Medical School. Care was taken in mating the control series to avoid any approach to inbreeding. As in the case of inbred rats the young were taken from the mother at the age of thirty days. The system of numbering individual control rats for identification was as follows: the first letter, S, signified that it was a standard or normal control rat, letters within parentheses gave the pedigree, and the figures gave the individual number. For example, S(C/EB)70♂ may be analyzed as follows: standard, or normal control series, Columbia father, Hopkins Medical maternal grandfather, maternal grandmother purchased from a Baltimore dealer, individual 70, male. The same system of ear marking was used as in the case of the inbred rats.

When taken from the mother males and females were kept in separate cages. According to Watson[3] the bearing of young has some effect upon the central nervous system of the white rat; for this reason, and in order to keep conditions constant, neither males nor females used in these experiments were allowed to mate. As solitude may be a factor affecting behavior,

[3] Watson: The Effect of the Bearing of Young Upon the Body-Weight and the Weight of the Central Nervous System of the Female White Rat. *Journ. of Comp. Neur. and Psych.*, Vol. XV., No. 6, 1905.

from three to five rats were kept in each cage, the cages being made sufficiently large (24″ x 15″ x 15″) to permit it. Cages were frequently disinfected with a preparation the principal ingredient of which is carbolic acid. The rats were occasionally immersed in a solution of this preparation in order to destroy skin parasites. A layer of clean chips and shavings was kept on the floor of all cages. The food, from the time of weaning, consisted of bread soaked in milk every day, grain and sunflower seeds twice a week, and banana or carrot once a fortnight. Temperature was kept as uniformly as possible at 70° F. In order to facilitate this a small gas heater was installed and it proved very efficient even during the coldest days of winter. As the animal laboratory is located in the basement the temperature, during the summer, rarely rose above our norm.

At the age of sixty days the rats intended for experimental purposes were placed on a short allowance of feeding time (thirty minutes) in order to prepare them for experimentation. The experiments were begun with both inbreds and normal control at the age of seventy days. Care was taken in each experiment to use the same number of males and females in the control series as in the inbred; this was necessary because, as in man, the relative brain weight of the female is greater than that of the male. Experiments upon individual rats were conducted as nearly as possible at the same time of day, both to form feeding rhythms and in order not to interfere with other rhythms.

There are three methods of estimating perfection in experiments relating to the habit-forming abilities of animals: the number of errors, the distance traversed, the time consumed. It is hard, in any case, to form a judgment as to what constitutes an error in the behavior of an animal; especially is this true in a comparative study of this kind where it is possible for the personal prejudices of the experimenter to become a factor. At the time this investigation was begun there was no adequate means of measuring the distance traversed. This left at the disposal of the experimenter but one criterion: the time consumed. However, time consumed in learning is the criterion most frequently used by experimenters in the animal field.

Hicks,[4] in summing up the experimental results of several investigators, concludes that "time is the best single criterion, inasmuch as it represents all phases of the process of learning, and since it will yield the most comparable results at the hands of different investigators." In timing the rats a very accurate Swiss split-second stopwatch was used. Under ideal conditions, perhaps, the animal should be presented to the problem by one person, timed by another, while the experimenter himself should merely record the results. But timing very soon becomes automatic; when the rat is crossing the line it is almost impossible to inhibit the impulse to press the stem of the watch.

At the conclusion of the experiments all the rats were shipped to the Wistar Institute where Dr. Shinkishi Hatai ascertained the anatomical data necessary for the formulation of the comparison between relative brain weight and the ability to form habits.

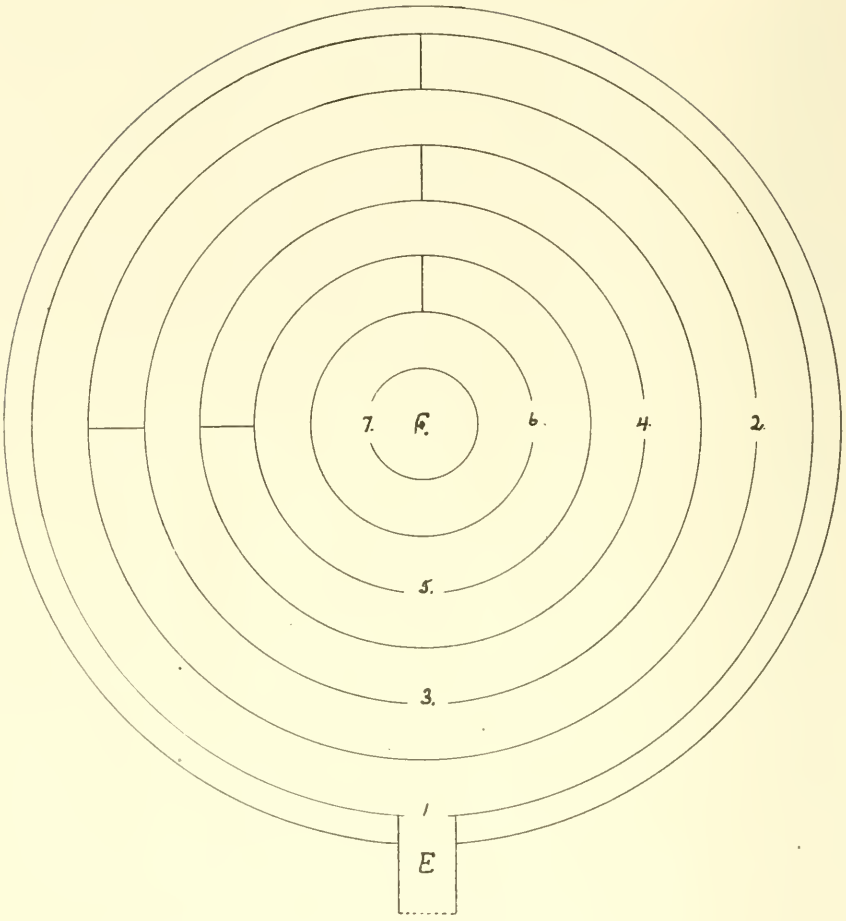
III. EXPERIMENT 1: THE MAZE

The apparatus used in this experiment was the Watson Maze (see Plate I). This maze is circular in form, five feet in diameter, with entrances from outer runways to the next inner at alternate ends of a quadrant arc. The runways are each four inches wide, and the centre, F, eight inches in diameter. The partitions are of aluminum and rise to a height of five inches above the floor of the maze. A heavy wire screen resting on the top serves the purpose of preventing the rats from climbing over the partitions, and also allows the experimenter to observe all movements within. The perfect course of the animal running is, from the entrance, E, through runway entrances 1, 2, 3, 4, 5, 6, and 7 to F (food). Each side of runway entrances 2 to 6 inclusive lead into *cul-de-sacs*.

The object of the experiment was to have each rat learn to reach the centre, F, in the least possible time, the starting time being taken when the animal crossed runway entrance 1, and the finishing time when he crossed entrance 7.

In preparation, each animal, beginning at the age of sixty-five days, was fed alone in the centre, F, ten minutes daily for

[4] Hicks, The Relative Values of the Different Curves of Learning. *Jour. Animal Behavior.*, Vol. I, pp. 138 ff.



The Watson Maze.

Plate I.

five consecutive days. During this period the centre was barred from the rest of the maze at entrance 6. At the age of seventy days the experiment began. Eleven males and ten females from the inbred strain were used and, as control, an equal number of males and females from the normal control series. Of the inbred rats, fourteen were from the sixth generation and seven from the seventh. The stimulus used was the food to which they had become accustomed, bread soaked in milk.

From the beginning of the experiment each rat was required to run from E to F five times daily. At the end of the fifth trial it was allowed to feed in the centre, F, for five minutes, but permitted no more food until the completion of the next day's experiment. Each rat was used daily until it had learned the course perfectly, the criterion of perfection being five perfect trials for each of three successive days. A perfect trial consisted in running the course within six seconds, a period of time so short that it was practically impossible for the rat to make a detectable error and reach the centre within that time. Those rats failing to learn within one hundred days (five hundred trials) were no longer used for experimentation. Such rats as learned the maze were, at the conclusion of the experimentation, fed for sixty days in a runway twenty-five feet long with a feeding-box at the right of the far end. At the end of this period they were tested for retention and relearning.

The shortest period of time required by an inbred rat to learn the maze perfectly was twelve days; for a control rat, ten days. Two inbred rats and one control failed to learn the maze habit within the one hundred days allowed. During the process of learning certain of the normal control series showed peculiarities of behavior similar to those exhibited by the inbred strains. These peculiarities, for the greater part, consisted in disorientation and persistent errors. All the normal control series, with the exception of five rats containing germ-plasm of the B strain, had perfected the maze habit by the twenty-fourth day. The control rat mentioned above as having failed to learn the maze within one hundred days was from this group. So erratic in behavior and so slow in learning were the B strain rats that the investigator suspected them to be of less than normal brain weight; and, when the returns were received from the Wistar Institute, this was indeed found to be the case.

For greater convenience in making a comparative study, I have placed together in Table I a summary consisting of the daily averages of the entire inbred group and, directly beneath, the corresponding daily averages of the entire normal control group. From this table, too, are constructed the comparative curves of learning.

Table I compares: (a) the progress of learning by days; (b) the "absolute retention" (this being a term used here to represent the time required to complete the first trial of the relearning series after the sixty days' rest; the greater the retention, the less is the time); (c) the progress of relearning by days; (d) the anatomical data.

Table Ia shows that two of the inbred and one of the control rats (the latter from the B strain) failed to learn the maze habit. The inbreds required, on the average, 36.62+ days to learn; the control but 24.67+ days. The absolute retention of the inbred rats (Table Ib) was, on the average, 81.558 seconds; of the control series, 59.640 seconds. The two inbreds and one control failing to learn the maze were not, of course, tested for retention and relearning. Of the inbreds so tested (Table Ic), two failed to relearn within fifty days, in consequence of which it was thought useless to carry them further. All the control series had relearned at the end of twenty-two days. The inbreds required, on the average, 12.68+ days to relearn; the normals but 5.75 days.

In all these criteria of ability: learning, absolute retention, and relearning, the rats of the normal control series are shown, on the average, to be superior to those of the inbred series.

There are two methods in use for estimating the relative brain weight: in reference to body length and in reference to body weight. In a healthy normal rat the difference between body weight in grams and body length in millimeters is slight; but, under conditions of overfeeding, underfeeding, or of sickness the body weight varies greatly while the body length remains constant. For this reason Dr. Donaldson of the Wistar Institute has accepted body length as the better method. I have laid greater stress on the body length criterion, although both are presented in the tables of anatomical data. Both body length and body weight of the inbred rats used in the maze (Table Id) are, on the average, slightly greater than is

the case with the normal controls. The relative brain weight (in reference to body length) of the inbreds is 4.43% less than that of the normals. The relative brain weight (in reference to body weight) of the inbreds is 7.99% less than that of the normal control. The percentage of water in brain and cord normally decreases with age; but in the inbreds used in the maze experiment, although killed, on the average, fourteen days later than the control rats, the percentage of water was greater.

The figures presented in Table I support the hypothesis that a less than normal average brain weight in a strain of rats is accompanied by an average lesser ability to form habits.

TABLE Ia

THE MAZE

DAILY LEARNING AVERAGES OF INBRED AND NORMAL CONTROL RATS
(Time in seconds)

	Day 1	Day 2	Day 3	Day 4	Day 5
Inbred Average	531.665	91.404	68.160	39.459	25.568
Control Average	505.128	110.739	53.851	28.404	25.613
	Day 6	Day 7	Day 8	Day 9	Day 10
Inbred Average	20.015	13.899	11.061	9.709	9.522
Control Average	17.366	13.381	11.994	12.440	9.619
	Day 11	Day 12	Day 13	Day 14	Day 15
Inbred Average	10.217	7.937	8.708	7.600	7.000
Control Average	7.295	7.748	7.354	6.904	7.277
	Day 16	Day 17	Day 18	Day 19	Day 20
Inbred Average	6.439	6.585	6.305	6.492	6.458
Control Average	6.687	6.428	5.900	6.209	5.851
	Day 21	Day 22	Day 23	Day 24	Day 25
Inbred Average	6.362	5.749	5.978	5.753	6.248
Control Average	5.630	5.816	5.710	5.675	5.420
	Day 26	Day 27	Day 28	Day 29	Day 30
Inbred Average	5.734	7.130	5.669	6.387	5.697
Control Average	5.389	5.442	5.479	5.496	5.437
	Day 31	Day 32	Day 33	Day 34	Day 35
Inbred Average	5.384	5.708	5.702	6.084	5.731
Control Average	5.700	5.502	5.378	5.499	5.308
	Day 36	Day 37	Day 38	Day 39	Day 40
Inbred Average	5.590	5.476	5.494	5.540	5.848
Control Average	5.272	5.249	5.327	5.363	5.316
	Day 41	Day 42	Day 43	Day 44	Day 45
Inbred Average	5.640	5.978	5.631	5.526	13.456
Control Average	5.343	5.573	5.375	5.250	5.444

TABLE Ia—Continued

	Day 46	Day 47	Day 48	Day 49	Day 50
Inbred Average	6.734	7.400	6.602	5.713	5.840
Control Average	5.282	5.162	5.147	5.198	5.223
	Day 51	Day 52	Day 53	Day 54	Day 55
Inbred Average	5.844	5.522	5.353	5.416	5.707
Control Average	5.322	5.360	5.192	5.615	5.261
	Day 56	Day 57	Day 58	Day 59	Day 60
Inbred Average	5.924	5.640	5.458	5.621	6.740
Control Average	5.286	5.181	5.358	5.286	5.257
	Day 61	Day 62	Day 63	Day 64	Day 65
Inbred Average	8.347	6.442	6.177	5.425	5.686
Control Average	5.398	5.952	5.743	5.299	5.288
	Day 66	Day 67	Day 68	Day 69	Day 70
Inbred Average	5.880	5.630	5.442	5.821	5.396
Control Average	5.345	5.339	5.173	5.360	5.223
	Day 71	Day 72	Day 73	Day 74	Day 75
Inbred Average	5.419	5.737	5.457	5.587	5.928
Control Average	5.244	5.170	5.130	5.206	5.110
	Day 76	Day 77	Day 78	Day 79	Day 80
Inbred Average	5.686	5.798	5.817	5.627	5.432
Control Average	5.183	5.288	5.170	5.143	5.421
	Day 81	Day 82	Day 83	Day 84	Day 85
Inbred Average	6.095	7.335	5.379	5.535	6.345
Control Average	5.263	5.236	5.211	5.208	5.160
	Day 86	Day 87	Day 88	Day 89	Day 90
Inbred Average	5.495	5.316	5.560	5.421	7.290
Control Average	5.288	5.156	5.181	5.152	5.152
	Day 91	Day 92	Day 93	Day 94	Day 95
Inbred Average	5.829	7.015	6.204	6.011	5.556
Control Average	5.257	5.095	5.141	5.210	5.166
	Day 96	Day 97	Day 98	Day 99	Day 100
Inbred Average	5.893	5.665	6.017	5.973	5.958
Control Average	5.217	5.149	5.118	5.187	5.215

	Failed to learn	Days required to learn
Inbr d Average	2	36.62+
Control Average	1	24.67+

TABLE Ib

THE MAZE

AVERAGE ABSOLUTE RETENTION OF INBRED AND NORMAL CONTROL RATS

	Absolute Retention after 60 days' rest
Inbred Average	81.558 seconds
Control Average	59.640 seconds

TABLE Ic

THE MAZE

DAILY RELEARNING AVERAGES OF INBRED AND NORMAL CONTROL RATS
(Time in seconds)

	Day 1	Day 2	Day 3	Day 4	Day 5
Inbred Average	35.415	12.208	10.069	9.560	8.069
Control Average	28.574	18.752	9.530	7.996	6.548
	Day 6	Day 7	Day 8	Day 9	Day 10
Inbred Average	7.672	7.659	6.642	6.232	6.604
Control Average	7.076	6.064	5.922	5.670	5.630
	Day 11	Day 12	Day 13	Day 14	Day 15
Inbred Average	6.200	5.966	6.067	5.660	5.587
Control Average	5.508	5.434	5.430	5.414	5.468
	Day 16	Day 17	Day 18	Day 19	Day 20
Inbred Average	5.634	5.669	5.680	6.029	5.718
Control Average	5.970	5.424	5.490	5.354	5.440
	Day 21	Day 22	Day 23	Day 24	Day 25
Inbred Average	6.046	5.834	5.844	6.061	5.771
Control Average	5.614	5.300	5.300	5.300	5.300
	Day 26	Day 27	Day 28	Day 29	Day 30
Inbred Average	6.166	5.697	5.914	5.842	5.817
Control Average	5.300	5.300	5.300	5.300	5.300
	Day 31	Day 32	Day 33	Day 34	Day 35
Inbred Average	5.901	5.905	5.640	5.846	5.939
Control Average	5.300	5 300	5.300	5.300	5.300
	Day 36	Day 37	Day 38	Day 39	Day 40
Inbred Average	5.920	5.956	5.903	5.766	5.726
Control Average	5.300	5.300	5.300	5.300	5.300
	Day 41	Day 42	Day 43	Day 44	Day 45
Inbred Average	5.657	5.848	5.779	5.745	5.861
Control Average	5.300	5.300	5.300	5.300	5.300
	Day 46	Day 47	Day 48	Day 49	Day 50
Inbred Average	6.032	5.815	5.920	5.762	5.697
Control Average	5.300	5.300	5.300	5.300	5.300

	Failed to relearn	Days required to relearn
Inbred Average	2	12.68+
Control Average	0	5.75

TABLE Id

THE MAZE

ANATOMICAL DATA OF INBRED AND NORMAL CONTROL RATS

	Body Length in mm.	Body Weight in grms.	Brain Weight in grms.	Cord Weight in grms.	Water in Brain per cent
Inbred Average	195.38	180.55	1.71122	.52852	78.497
Control Average	191.00	171.41	1.74930	.52740	78.319

	Water in Cord per cent	Per cent Brain Weight in Relation to Body Length	Per cent Brain Weight in Relation to Body Weight	Age killed Days
Inbred Average	71.723	.87685	.97052	200
Control Average	71.666	.91745	1.05479	186

In Plate II is shown the curve of learning (below) and of relearning (above) of the inbred rats compared with those of the normal control. These curves are constructed from figures given in Table I. The curve of the inbred rats is indicated by the solid line, that of the normal control by the broken line. The ordinates give the average daily time in seconds for each group, and the abscissae the number of the day in which such time was made. The time required by both inbred and control rats for the first four days was so long that it is represented here by figures and does not appear in the curve. For the first few days the descent in time for both the inbreds and the control is very rapid. From the twentieth day the curve of the control rats lies entirely below the six-second mark. The curve of the inbred rats never reaches even an approximately flattened appearance, but exhibits great irregularities, particularly on the forty-fifth, sixty-first, eighty-second, ninetieth and ninety-second days. The inbreds' curve of relearning is more similar to that of the controls, but it must be borne in mind that the two inbreds and one control rat that failed to learn the maze are not represented in the relearning curve, and for this reason this curve applies to selected groups. From the twenty-second day the control curve of relearning is perfectly flat at 5.3 seconds, all the control rats having relearned. Two of the inbred rats having failed to relearn, their curve of relearning remains slightly irregular and above that of the control in time.

In Plate III may be seen the curves representing the distribution of learning and relearning of both inbreds and control for the maze experiment. The time is given in days—in groups of five for learning, in groups of two for relearning. As may readily be seen, the advantage from the standpoint of time (days required to learn and relearn) lies wholly in favor of the normal control group.

The question arises as to whether the later generations of inbred rats differ from the earlier in the ability to form habits; that is, is decrease in this ability progressive even if, as earlier stated, decrease in relative brain weight after the 4th generation is not. Of the inbred rats used in the maze experiment, fourteen were from the 6th generation and seven from the 7th generation. In Table II is presented a comparative summary consisting of the daily averages of the 6th and 7th generation

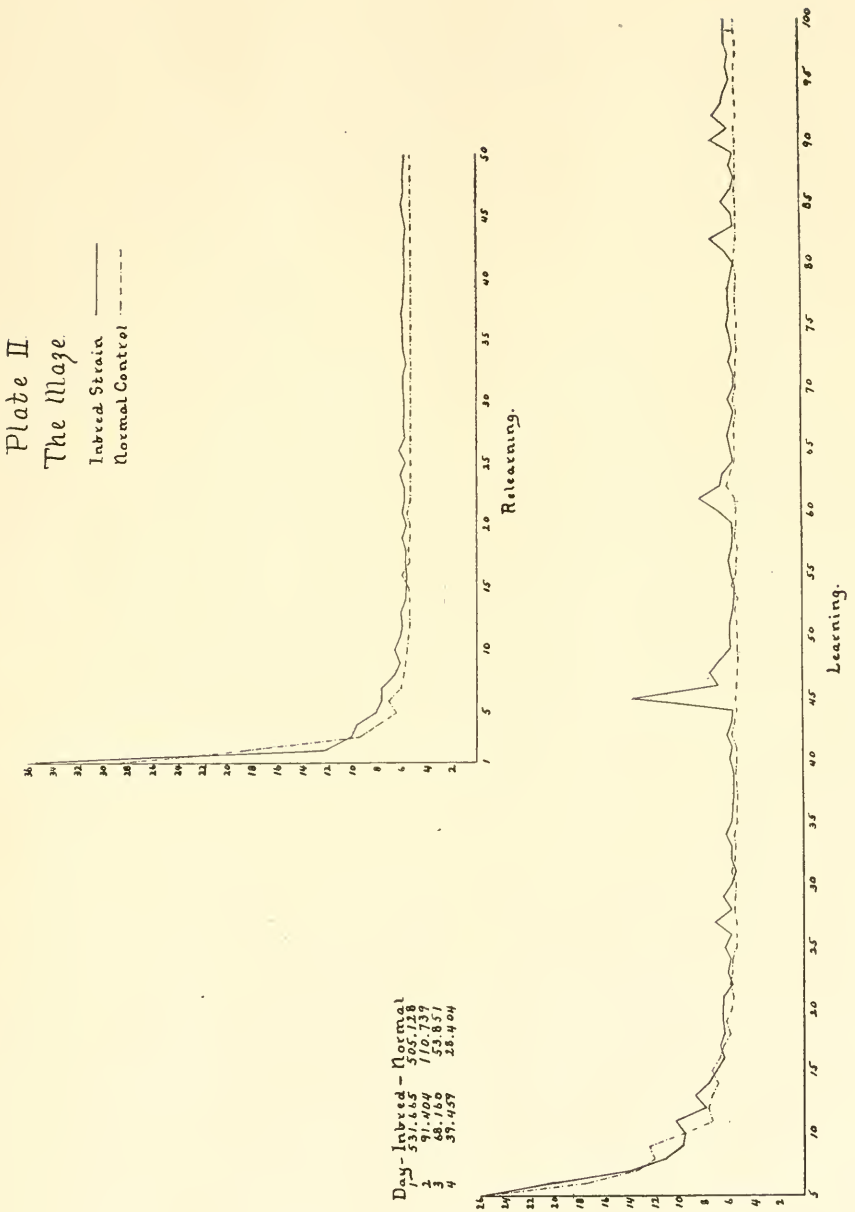


Plate II
The Maze
Inbred Strain
Normal Control
Day - Inbred - Normal
1 531.665 505.128
2 91.404 110.739
3 68.160 53.851
4 39.459 28.404
Relearning.
Learning.

Plate III.

The Maze.

Distribution of Learning and Relearning.

inbred rats used in this experiment. Two of the 7th generation failed to learn the maze; all the 6th generation had learned it after eighty-three days. The 6th generation required, on the average, 32.93 days in which to learn; the 7th generation, 44.00+ days. The absolute retention of the 6th generation was, on the average, 65.443 seconds; of the 7th generation, 126.680 seconds. Two of the 6th generation failed to relearn; all the 7th generation had relearned at the end of fourteen days. The 6th generation required, on the average, 14.14+ days to relearn; the 7th generation but 8.60 days.

In these criteria of ability, the 6th generation excelled in learning and absolute retention; the 7th in relearning. It must be remembered, however, that the 7th generation rats used in the relearning test formed a selected group, the two rats having failed to learn having been from this generation. On the whole, the ability of the 7th generation inbreds in the maze experiment appears to be somewhat inferior to that of the 6th generation.

The body length and body weight of the 6th generation average greater than those of the 7th. The average actual brain weight of the 6th generation is greater than that of the 7th. The relative brain weight (in reference to body length) of the 6th generation is .91% less than that of the 7th generation. The relative brain weight (in reference to body weight) of the 6th generation is 1.50% less than that of the 7th generation. The relative brain weight of the inbred rats used in the maze has not decreased from one generation to the next; but all the 7th generation rats were females, and the females normally have relatively greater brain weights than the males. The percentage of water in brain and cord is within .03% of the same figure in the two generations.

TABLE IIa

THE MAZE

DAILY LEARNING AVERAGES OF SIXTH AND SEVENTH GENERATION INBRED RATS
(Time in seconds)

	Day 1	Day 2	Day 3	Day 4	Day 5
Sixth Average.....	541.423	73.343	79.997	37.449	30.014
Seventh Average.....	512.149	127.526	44.486	43.480	16.674
	Day 6	Day 7	Day 8	Day 9	Day 10
Sixth Average.....	24.457	16.371	10.646	10.486	9.789
Seventh Average.....	11.131	8.954	11.891	8.154	8.989

TABLE IIa—(Continued)

	Day 11	Day 12	Day 13	Day 14	Day 15
Sixth Average	11.157	8.294	9.825	7.958	7.130
Seventh Average	8.337	7.223	6.760	6.926	6.741
	Day 16	Day 17	Day 18	Day 19	Day 20
Sixth Average	6.767	6.690	6.350	6.376	6.421
Seventh Average	5.781	6.376	6.216	6.724	6.530
	Day 21	Day 22	Day 23	Day 24	Day 25
Sixth Average	6.276	5.741	5.899	5.576	6.004
Seventh Average	6.536	5.764	6.136	6.107	6.736
	Day 26	Day 27	Day 28	Day 29	Day 30
Sixth Average	5.604	5.716	5.455	5.441	5.466
Seventh Average	5.993	9.959	6.096	8.279	6.159
	Day 31	Day 32	Day 33	Day 34	Day 35
Sixth Average	5.320	5.674	5.594	5.658	5.464
Seventh Average	5.513	5.776	5.919	6.936	6.267
	Day 36	Day 37	Day 38	Day 39	Day 40
Sixth Average	5.406	5.444	5.284	5.316	5.236
Seventh Average	5.959	5.541	5.913	5.987	7.073
	Day 41	Day 42	Day 43	Day 44	Day 45
Sixth Average	5.399	5.853	5.396	5.261	5.461
Seventh Average	6.124	6.227	6.101	6.056	29.444
	Day 46	Day 47	Day 48	Day 49	Day 50
Sixth Average	5.364	5.259	5.361	5.396	5.387
Seventh Average	9.473	11.684	9.084	6.347	6.747
	Day 51	Day 52	Day 53	Day 54	Day 55
Sixth Average	5.713	5.447	5.230	5.244	5.647
Seventh Average	6.107	5.673	5.599	5.759	5.827
	Day 56	Day 57	Day 58	Day 59	Day 60
Sixth Average	5.336	5.359	5.301	5.210	5.241
Seventh Average	7.101	6.204	5.770	6.444	9.736
	Day 61	Day 62	Day 63	Day 64	Day 65
Sixth Average	5.333	5.366	5.312	5.198	5.209
Seventh Average	14.376	8.593	7.907	5.879	6.639
	Day 66	Day 67	Day 68	Day 69	Day 70
Sixth Average	5.198	5.264	5.186	5.286	5.166
Seventh Average	7.244	6.364	5.953	6.890	5.856
	Day 71	Day 72	Day 73	Day 74	Day 75
Sixth Average	5.158	5.184	5.192	5.158	5.212
Seventh Average	5.941	6.844	5.987	6.444	7.359
	Day 76	Day 77	Day 78	Day 79	Day 80
Si th Average	5.249	5.189	5.209	5.269	5.169
Seventh Average	6.559	7.016	7.033	6.341	5.959
	Day 81	Day 82	Day 83	Day 84	Day 85
Sixth Average	5.155	5.149	5.158	5.153	5.158
Seventh Average	7.976	11.707	5.821	6.290	8.730

TABLE IIa—(Continued)

	Day 86	Day 87	Day 88	Day 89	Day 90
Sixth Average	5.158	5.158	5.158	5.158	5.158
Seventh Average	6.170	5.633	6.364	5.947	11.553
	Day 91	Day 92	Day 93	Day 94	Day 95
Sixth Average	5.158	5.158	5.158	5.158	5.158
Seventh Average	7.170	10.730	8.296	7.719	6.353
	Day 96	Day 97	Day 98	Day 99	Day 100
Sixth Average	5.158	5.158	5.158	5.158	5.158
Seventh Average	7.364	6.679	7.736	7.604	7.559

	Failed to learn	Days required to learn
Sixth Average	0	32.93
Seventh Average	2	44.00+

TABLE IIb

THE MAZE

AVERAGE ABSOLUTE RETENTION OF SIXTH AND SEVENTH GENERATION INBRED RATS

	Absolute Retention after 60 Days' Rest
Sixth Average	65.443 seconds
Seventh Average	126.680 seconds

TABLE IIc

THE MAZE

DAILY RELEARNING AVERAGES OF SIXTH AND SEVENTH GENERATION INBRED RATS
(Time in seconds)

	Day 1	Day 2	Day 3	Day 4	Day 5
Sixth Average	31.866	10.357	11.043	9.009	8.071
Seventh Average	45.352	17.392	7.344	11.104	8.064
	Day 6	Day 7	Day 8	Day 9	Day 10
Sixth Average	8.174	7.426	6.351	6.111	6.129
Seventh Average	6.264	8.312	7.456	6.584	7.936
	Day 11	Day 12	Day 13	Day 14	Day 15
Sixth Average	6.163	6.054	5.886	5.660	5.563
Seventh Average	6.304	5.720	6.576	5.656	5.656
	Day 16	Day 17	Day 18	Day 19	Day 20
Sixth Average	5.628	5.674	5.689	6.163	5.740
Seventh Average	5.656	5.656	5.656	5.656	5.656
	Day 21	Day 22	Day 23	Day 24	Day 25
Sixth Average	6.186	5.897	5.911	6.206	5.811
Seventh Average	5.656	5.656	5.656	5.656	5.656
	Day 26	Day 27	Day 28	Day 29	Day 30
Sixth Average	6.349	5.711	6.006	5.909	5.874
Seventh Average	5.656	5.656	5.656	5.656	5.656

TABLE IIc—(Continued)

	Day 31	Day 32	Day 33	Day 34	Day 35
Sixth Average	5.989	5.994	5.634	5.914	6.040
Seventh Average	5.656	5.656	5.656	5.656	5.656
	Day 36	Day 37	Day 38	Day 39	Day 40
Sixth Average	6.014	6.063	5.991	5.806	5.751
Seventh Average	5.656	5.656	5.656	5.656	5.656
	Day 41	Day 42	Day 43	Day 44	Day 45
Sixth Average	5.657	5.917	5.823	5.777	5.934
Seventh Average	5.656	5.656	5.656	5.656	5.656
	Day 46	Day 47	Day 48	Day 49	Day 50
Sixth Average	6.166	5.871	6.014	5.800	5.711
Seventh Average	5.656	5.656	5.656	5.656	5.656

	Failed to Relearn	Days Required to Relearn
Sixth Average	2	14.14+
Seventh Average	0	8.60

TABLE IId

THE MAZE

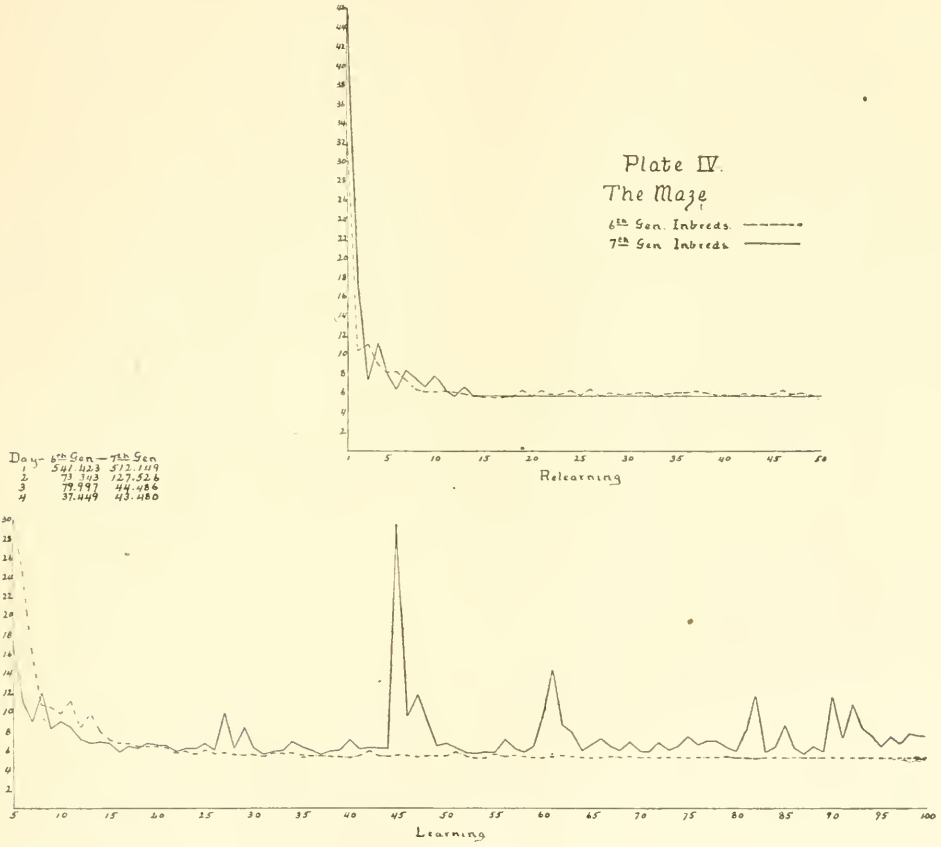
ANATOMICAL DATA OF SIXTH AND SEVENTH GENERATION INBRED RATS

	Body Length in mm.	Body Weight in grms.	Brain Weight in grms.	Cord Weight in grms.	Water in Brain per cent
Sixth Average	200.71	195.04	1.75234	.53881	78.49
Seventh Average	184.71	152.47	1.63286	.50794	78.51

	Water in Cord per cent	Per cent Brain Weight in Relation to Body Length	Per cent Brain Weight in Relation to Body Weight	Age killed Days
Sixth Average	71.73	.87418	.91653	196
Seventh Average	71.70	.88219	1.07851	207

In Plate IV is shown the curve of learning (below) and of relearning (above) of the 6th generation of inbred rats compared with those of the 7th. These curves are constructed from figures given in Table II. The curve of the 6th generation rats is indicated by the broken line, that of the 7th generation by the solid line. The ordinates show the average daily time in seconds for each group, and the abscissae the number of the day in which such time was made. From the twenty-second day the curve of learning of the 6th generation lies below the six-second mark, and from the eighty-third day is flat at 5.158 seconds, signifying that on that day the last of this group had perfected the habit. The 7th generation learning curve is very irregular

throughout its length and never approaches the appearance of perfect learning. The 7th generation relearning curve, however, is slightly better than that of the 6th, being flat from the fourteenth day at 5.656 seconds. But it is again necessary to call attention to the fact that this was a selected group, the two



rats failing to learn having been thrown out and not tested for relearning.

The similarity in behavior of the rats of the control series containing blood of the B strain (of which the original parents were purchased from a Baltimore dealer) to the behavior of the inbreds has already been mentioned. The length of time re-

quired by them to learn the maze had led the investigator to suspect a less than normal brain weight; and, when the brains were weighed, this was found to be the case. Table III presents a comparative summary consisting of the daily averages of the nine rats containing B blood and of the twelve control rats lacking it. Eight of the rats containing B are one-half C and one-half B; the remaining rat is one-half C, one-fourth E, and one-fourth B. That the C blood is not a factor in their erratic behavior is proven by the fact that most of the rats of the control series not containing B blood do also contain C. In order to compare the behavior of control rats having B and those lacking it with that of the inbred rats, cross reference may be made from Table III to the inbred averages of Table I. The control rats having B blood shall be referred to in Table III as Control +B; those lacking B blood as Control —B.

The tables (I and III) show that two of the inbreds and one of the +B failed to learn the maze; the —B controls had all learned at the end of the twenty-fifth day. The inbred rats required, on the average, 36.62+ days to learn; the +B 35.67+ days, and the —B but 16.42 days. The absolute retention of the inbreds was, on the average, 81.558 seconds; of the +B, 72.475 seconds; and of the —B, but 51.083 seconds. Two of the inbreds failed to relearn; all the +B had relearned at the end of the twenty-second day; while all the —B had relearned at the end of the eighth day. The inbreds required, on the average, 12.68+ days to relearn; the +B, 8.24 days; the —B, but 4.08 days.

In these criteria of ability to learn the maze, the inbred rats did the least well; the +B rats were, in each instance, above, but not far from, the record of the inbreds; the —B were much superior to either.

Both body length and body weight were greatest in the inbreds, next in the +B, and least in the —B. Actual brain weight was least in the inbreds, much greater in the +B, and slightly greater in the —B than in the +B. The relative brain weight (in reference to body length) of the inbreds was 5.46% less than that of the —B; that of the +B was 2.53% less than that of the —B. The relative brain weight (in reference to body weight) of the inbred rats was 10.02% less than that of the —B; that of the +B was 5.15% less than that of the —B. As might

be expected, from our hypothesis and the behavior, the average relative brain weight of the +B rats lies between that of the inbreds and of the —B.

The results obtained from the supposedly normal B rats reinforce the former conclusion that a lesser relative brain weight is accompanied in a similar degree by a lesser ability to form habits.

TABLE IIIa

THE MAZE

DAILY LEARNING AVERAGES OF +B AND —B NORMAL CONTROL RATS
(Time in seconds)

	Day 1	Day 2	Day 3	Day 4	Day 5
Control +B	849.458	187.729	88.511	36.160	29.689
Control —B	248.380	52.997	27.857	15.085	22.557
	Day 6	Day 7	Day 8	Day 9	Day 10
Control +B	20.093	15.822	13.066	10.276	12.964
Control —B	15.320	11.550	11.223	14.063	7.110
	Day 11	Day 12	Day 13	Day 14	Day 15
Control +B	8.142	9.544	8.907	7.476	9.289
Control —B	6.652	6.393	6.190	6.475	5.768
	Day 16	Day 17	Day 18	Day 19	Day 20
Control +B	6.702	7.822	7.067	7.467	7.040
Control —B	6.675	5.382	5.025	5.265	4.959
	Day 21	Day 22	Day 23	Day 24	Day 25
Control +B	6.529	6.973	6.804	6.719	6.102
Control —B	4.956	4.948	4.888	4.892	4.908
	Day 26	Day 27	Day 28	Day 29	Day 30
Control +B	6.031	6.156	6.240	6.280	6.142
Control —B	4.908	4.908	4.908	4.908	4.908
	Day 31	Day 32	Day 33	Day 34	Day 35
Control +B	6.756	6.294	6.006	6.287	5.842
Control —B	4.908	4.908	4.908	4.908	4.908
	Day 36	Day 37	Day 38	Day 39	Day 40
Control +B	5.758	5.704	5.887	5.971	5.861
Control —B	4.908	4.908	4.908	4.908	4.908
	Day 41	Day 42	Day 43	Day 44	Day 45
Control +B	5.923	6.461	5.999	5.706	6.159
Control —B	4.908	4.908	4.908	4.908	4.908
	Day 46	Day 47	Day 48	Day 49	Day 50
Control +B	5.791	5.501	5.466	5.586	5.643
Control —B	4.908	4.908	4.908	4.908	4.908
	Day 51	Day 52	Day 53	Day 54	Day 55
Control +B	5.874	5.963	5.572	6.559	5.732
Control —B	4.908	4.908	4.908	4.908	4.908
	Day 56	Day 57	Day 58	Day 59	Day 60
Control +B	5.679	5.546	5.959	5.690	5.723
Control —B	4.908	4.908	4.908	4.908	4.908

TABLE IIIa—(Continued

	Day 61	Day 62	Day 63	Day 64	Day 65
Control +B	6.052	7.346	6.857	5.821	5.794
Control —B	4.908	4.908	4.908	4.908	4.908
	Day 66	Day 67	Day 68	Day 69	Day 70
Control +B	5.928	5.914	5.528	5.963	5.643
Control —B	4.908	4.908	4.908	4.908	4.908
	Day 71	Day 72	Day 73	Day 74	Day 75
Control +B	5.692	5.519	5.426	5.603	5.381
Control —B	4.908	4.908	4.908	4.908	4.908
	Day 76	Day 77	Day 78	Day 79	Day 80
Control +B	5.550	5.794	5.519	5.457	6.106
Control —B	4.908	4.908	4.908	4.908	4.908
	Day 81	Day 82	Day 83	Day 84	Day 85
Control +B	5.746	5.673	5.617	5.608	5.497
Control —B	4.908	4.908	4.908	4.908	4.908
	Day 86	Day 87	Day 88	Day 89	Day 90
Control +B	5.794	5.488	5.546	5.479	5.479
Control —B	4.908	4.908	4.908	4.908	4.908
	Day 91	Day 92	Day 93	Day 94	Day 95
Control +B	5.723	5.346	5.452	5.612	5.510
Control —B	4.908	4.908	4.908	4.908	4.908
	Day 96	Day 97	Day 98	Day 99	Day 100
Control +B	5.630	5.470	5.399	5.559	5.626
Control —B	4.908	4.908	4.908	4.908	4.908

	Failed to Learn	Days Required to Learn
Control +B	1	35.67+
Control —B	0	16.42

TABLE IIIb

THE MAZE

AVERAGE ABSOLUTE RETENTION OF +B AND —B NORMAL CONTROL RATS

	Absolute Retention After 60 Days' Rest
Control +B	72.475 seconds
Control —B	51.083 seconds

TABLE IIIc

THE MAZE

DAILY RELEARNING AVERAGES OF +B AND —B NORMAL CONTROL RATS

	Day 1	Day 2	Day 3	Day 4	Day 5
Control +B	36.215	29.230	10.930	11.180	7.990
Control —B	23.480	11.767	8.597	5.873	5.587
	Day 6	Day 7	Day 8	Day 9	Day 10
Control +B	8.075	7.200	6.970	6.340	6.240
Control —B	6.410	5.307	5.223	5.223	5.223

TABLE IIIc—(Continued)

	Day 11	Day 12	Day 13	Day 14	Day 15
Control +B	5.935	5.750	5.740	5.700	5.835
Control —B	5.223	5.223	5.223	5.223	5.223

	Day 16	Day 17	Day 18	Day 19	Day 20
Control +B	7.090	5.725	5.890	5.550	5.765
Control —B	5.223	5.223	5.223	5.223	5.223

	Day 21	Day 22 to Day 50
Control +B	6.200	5.415
Control —B	5.223	5.223

	Failed to Relearn	Days Required to Relearn
Control +B	0	8.24
Control —B	0	4.08

TABLE IIId

THE MAZE

ANATOMICAL DATA OF +B AND —B NORMAL CONTROL RATS

	Body Length in mm.	Body Weight in grms.	Brain Weight in grms.	Cord Weight in grms.	Water in Brain, per cent
Control +B	193.00	175.73	1.75378	.54191	78.25
Control —B	189.50	168.18	1.75428	.51645	78.37

	Water in Cord, Per cent	Per cent Brain Weight in Relation to Body Length	Per cent Brain Weight in Relation to Body Weight	Age killed, Days
Control +B	71.48	.90406	1.02303	200
Control —B	71.81	.92748	1.07861	175

In Plate V is shown the curve of learning (below) and of relearning (above) of the inbred and +B rats compared with those of the —B. The inbred curve is represented by the solid line, the +B by the heavy broken line, and the —B by the lighter broken line. The ordinates show the average daily time in seconds for each group, and the abscissae the number of the day in which such time was made. From the twentieth day the —B curve is flat at 4.9 seconds. Neither the inbred nor the +B curves flatten entirely, although the +B curve of learning is more regular than that of the inbreds. The curve of relearning (lacking the two inbreds and one +B that failed to learn) shows little difference between the three groups. But even here, although the inbred and +B are selected groups, the —B remains superior to both, and the +B is slightly superior to the inbred rats.

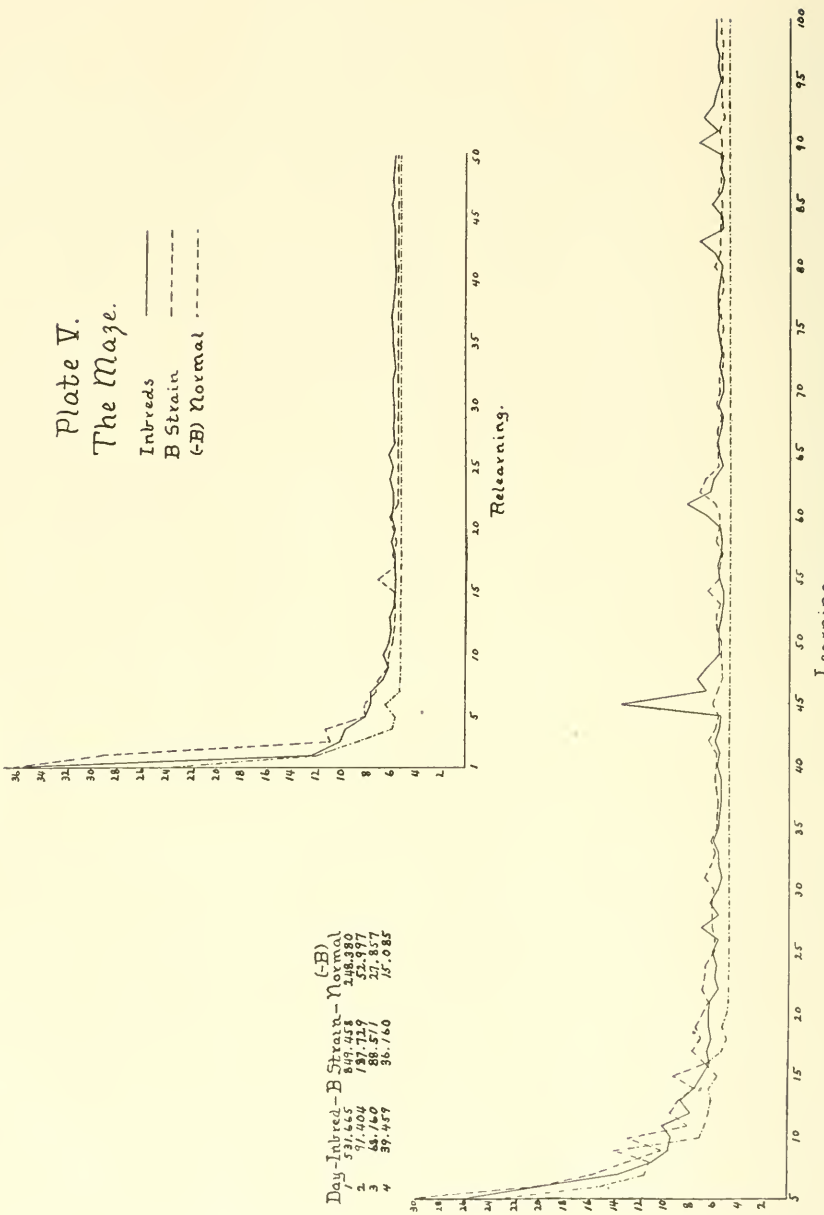


Plate V.
The Maze.
Inbreds
B Strain
(-B) Normal
Relearning.
Learning.
Day-Inbred-B Strain-(-B) Normal
1 51.665 69.458 148.889
2 31.665 19.458 57.889
3 44.760 38.571 27.857
4 39.459 36.760 17.085

IV. EXPERIMENT 2: THE PRELIMINARY INCLINED PLANE

The apparatus used in this experiment (see Plate VI) was designed especially to make a problem exceptionally difficult to learn, and in this purpose it exceeded expectations. The basic principle is the same as that of the apparatus designed and used

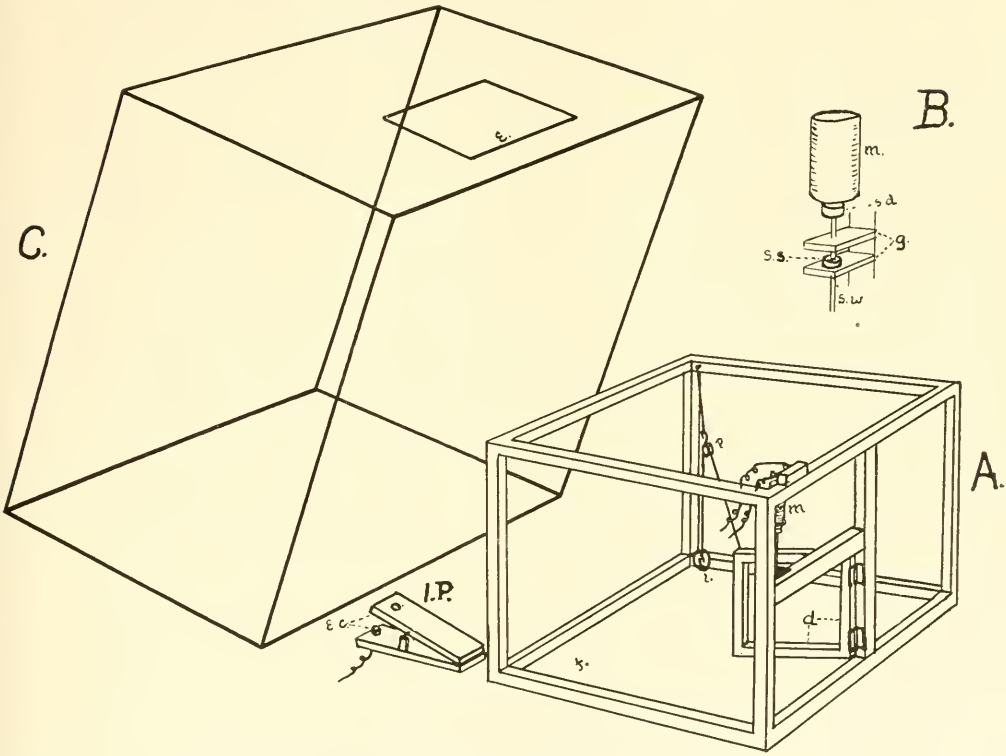


Plate VI. The Inclined Plane.

by Watson in his experiments at the University of Chicago, called by him the " Inclined Plane," and which is described and illustrated in his monograph "Animal Education," page 37.[5] But my apparatus differs from his in several respects.

Plate VI shows in detail the construction and method of operation. The food box, A, is framed of wood, eleven by

[5] Watson: Animal Education, Chicago, 1903.

twelve inches base, eleven inches in height, and is covered on top and sides with three-eighths inch heavy wire mesh. It is fitted with a hard rubber door, d, three-sixteenths inch thick, five inches high, and four and one-fourth inches wide. To the inner side of the door is fastened a cord which passes over a pulley, p, and is weighted at the other end with a piece of lead, l, of sufficient weight to insure the opening of the door upon releasing the latch. B shows the device for latching and releasing the door. A short distance above the door is fastened a three-inch electrical magnet, m; directly below that is a steel wire, s.w., surmounted by a steel disk, s.d., of the same diameter as the core of the magnet. The steel wire holds the door by dropping through holes in two brass plates, g, which serve as guides, to a point, behind another brass plate which is set at the top of and behind the door, one and one-half millimeters below the top. The setscrew, s.s., placed on the steel wire above the lower guide prevents any further drop. When the steel wire holds the door the disk is two mm. below the magnet; when the disk is drawn up to the magnet one-half mm. clearance is allowed for the door to swing back. Back of the feeding box, A, is placed the inclined plane, I.P.

The inclined plane has a hard rubber base three-eighths inch thick, six inches long, and two and three-eighths inches wide. Upon pivot standards rising from the middle of the base rests the plane itself. The plane is of wood fibre and of the same dimensions as the base. It is weighted at the end nearest the feeding box in order to insure its return to position after use. At the end opposite the weight and farthest from the feeding box, platinum electrical contacts, e.c., are placed in both base and plane. The power is provided through wires connecting the regular electric lighting system, 115 volts, direct current, with the wired apparatus. A 32 candle power lamp is placed in the series in order to avoid any danger of short-circuiting. To make the contact and allow the current to pass through the magnet, thus raising the steel wire and releasing the door, it is necessary for the rat to step on the point of operation, o, which lies well out toward the end of the plane. On account of a certain amount of latency in the operation of the magnet, the rat must not only make the contacts touch, but must also inhibit further action, remaining on the point of operation until

click of the disk meeting the magnet is heard. Over the food box and plane is placed a cage, C, constructed of one-half inch heavy wire mesh, the base measurements of which are twenty-four by twenty-four inches and the height fourteen inches. This allows the rat ample room to explore all sides of and above the food box. When the rat is placed within, the entrance, e, to the cage is closed.

The preliminary inclined plane experiment was not intended so much as a decisive experiment as to test the efficiency of the apparatus. The results, however, are significant and, therefore, included here.

The object of this experiment was to have each rat learn to reach the interior of the food box from the cage entrance in the least possible period of time. The procedure of a perfectly trained rat was to run from the entrance, e, to the point of operation, o, remaining there until the click of the disk against the magnet insured the door being open, then running through the door of the box to the food which was placed within at point f. The starting time was taken when the animal entered at e, another when the magnet clicked, and the final time when the food box was entered. The object of recording the two periods of time was that it had been anticipated that differences in association between the inbred series and the control rats might appear. But, as in both series the association was practically perfect by the third day, a comparison of such differences was thought useless.

In preparation for the experiment each animal, beginning at the age of sixty-five days, was fed alone in the food box, the door remaining open, ten minutes daily for five consecutive days. This gave the rat an opportunity to become acquainted with all parts of the interior of both box and cage, and also accustomed him to a reduced feeding time. At the age of seventy days the experiment began. Six males and five females from the inbred strain were used and, as control, an equal number of males and females from the normal series. All the inbred rats were from the 6th generation. The stimulus used was their regular food, bread soaked in milk.

As one of the first rats used consumed fourteen hours before his first accidental success, it was decided to use "cumulative time" for the first few trials. By this method each rat was

allowed to work thirty minutes and then, if unsuccessful, he was taken out, the food box door was opened, and he was then returned to feed for five minutes and used no more that day. When they began to succeed within the half hour, each rat was required to open and enter the food box five times daily. At the end of the fifth trial it was allowed to feed for five minutes, but was permitted no more food until the completion of the next day's experiment. Each rat was used daily for twenty days, making one hundred trials. As a time limit had been placed, no criterion of perfect learning was established for this experiment. At the conclusion of the learning experiment the rats were fed in the runway, which has already been described, for sixty days. At the end of this period they were tested for absolute retention and relearning, and were worked for five days, twenty-five trials, in order to ascertain the effects of the previous training.

In Table IV is a comparative summary consisting of the daily averages of the entire inbred group and, directly beneath, the corresponding daily averages of the entire normal control group.

From the eleventh day the daily time averages of the control rats are less than those of the inbreds. The absolute retention of the control rats is superior to that of the inbreds. In the five days allotted to testing the effects of previous training, the average time of the control rats is less each day than that of the inbreds.

In these criteria of ability the rats of the normal control series are shown to be, on the average, superior to those of the inbred series.

Body length of the inbred rats used in the preliminary inclined plane is, on the average, slightly greater than is the case with the control; body weight, however, is a trifle less. The average actual brain weight of the inbreds is less than that of the control. The relative brain weight (in reference to body length) of the inbreds is 11.61% less than that of the control. The relative brain weight (in reference to body weight) of the inbreds is 11.65% less than that of the control. Although killed at a later age, the percentage of water in brain and cord of the inbreds is greater than is the case with the control.

The preliminary inclined plane figures presented in this table (IV) support the hypothesis that a less than normal average brain weight in a strain of rats is accompanied by a lesser ability to form habits.

TABLE IVa

THE PRELIMINARY INCLINED PLANE

DAILY LEARNING AVERAGES OF INBRED AND NORMAL CONTROL RATS

(Time in seconds)

	Day 1	Day 2	Day 3	Day 4	Day 5
Inbred Average	1965.858	1631.178	60.302	45.916	28.116
Control Average	2470.393	971.102	79.775	27.564	21.840
	Day 6	Day 7	Day 8	Day 9	Day 10
Inbred Average	11.120	14.833	9.095	16.855	7.109
Control Average	20.305	10.375	9.869	8.971	8.058
	Day 11	Day 12	Day 13	Day 14	Day 15
Inbred Average	6.262	9.465	7.658	10.916	10.291
Control Average	5.342	5.015	5.055	4.425	4.513
	Day 16	Day 17	Day 18	Day 19	Day 20
Inbred Average	7.360	6.247	5.531	8.811	10.251
Control Average	5.062	3.727	4.800	5.815	5.622

TABLE IVb

THE PRELIMINARY INCLINED PLANE

AVERAGE ABSOLUTE RETENTION OF INBRED AND NORMAL CONTROL RATS.

	Absolute Retention After 60 Days' Rest
Inbred Average	59.309 seconds
Control Average	49.164 seconds

TABLE IVc

THE PRELIMINARY INCLINED PLANE

DAILY RELEARNING AVERAGES OF INBRED AND NORMAL CONTROL RATS

(Time in seconds)

	Day 1	Day 2	Day 3	Day 4	Day 5
Inbred Average	24.302	9.905	11.516	8.149	7.869
Control Average	17.102	5.498	5.869	4.262	6.651

TABLE IVd

THE PRELIMINARY INCLINED PLANE

ANATOMICAL DATA OF INBRED AND NORMAL CONTROL RATS

	Body Length in mm.	Body Weight in grms.	Brain Weight in grms.	Cord Weight in grms.	Water in Brain, per cent
Inbred Average	190.82	166.50	1.62031	.47535	78.565
Control Average	189.45	168.08	1.81946	.52819	77.982

	Water in Cord, Per cent	Per cent Brain Weight in Relation to Body Length	Per cent Brain Weight in Relation to Body Weight	Age killed, Days
Inbred Average	71.436	.84929	.98140	194
Control Average	71.128	.96084	1.09571	170

In Plate VII is shown the curve of learning (left) and of relearning (right) of the inbred rats compared with those of the normal control. These curves are constructed from figures given in Table IV. The curve of the inbred rats is indicated by the solid line, that of the normal control by the broken line. The ordinates give the average daily time in seconds for each group, and the abscissae the number of the day in which such time was made. The time required by both inbred and control rats for the first four days was so long that it is represented here by figures and does not appear in the curve. Both learning curves are irregular, but on the eleventh day that of the control passes permanently below that of the inbred. The curves of relearning show that the inbreds had failed to benefit by practice to so great an extent as the normal control.

V. EXPERIMENT 3: THE INCLINED PLANE

The apparatus used in this experiment was the same as that used in Experiment 2: the Preliminary Inclined Plane. The animals were prepared in the same way as for the previous experiment, and began work at the age of seventy days. Sixteen males and fourteen females from the inbred strain were used and, as control, an equal number of males and females from the normal series. Of the inbred rats, fifteen were from the seventh generation, fourteen from the eighth, and one from the ninth. As the behavior of the single ninth generation rat did not vary greatly from the average of the eighth generation, her results have been included in the tables and curves of the eighth generation. The stimulus used in this experiment was the same as in the two preceding, bread soaked in milk.

Cumulative time was used in recording the earlier trials as in the previous experiment. When the rats began to succeed in entering the food box within the half hour, each one was required to open and enter the food box three times each day. At the end of the third trial it was allowed to feed in the box for five minutes, but was permitted no more food until the completion of the next day's experiment. Each rat was used daily until it had learned the problem perfectly, the criterion of perfection being three perfect trials for each of three successive days. A perfect trial consisted in running from the entrance to the point of operation on the plane at the rear of the food box, opening the door, run-

Day	Inbred	Normal
1	1965.858	2470.393
2	1631.178	971.102
3	60.302	79.775
4	45.916	27.564

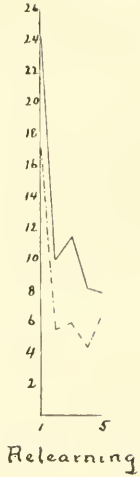


Plate VII.

Preliminary Inclined Plane.

Inbred Strain ——————

Normal Control - - - - - -

ning around and entering the box, all within four seconds; but, if the time consumed in opening the box after passing the entrance was more than two seconds, or if the time consumed in entering the box after having opened the door was more than two seconds, the trial was considered a failure. Thus it was possible for a rat to have a perfect trial in as long a total time as four seconds, or a failure in a less total time. Those rats failing to learn within one hundred days (three hundred trials) were no longer used for experimentation. Those rats learning the inclined plane were, at the conclusion of the experiment, fed for sixty days in the runway. At the end of this period they were tested for absolute retention and relearning.

Three of the rats formed the habit of *lifting* the plane at the end nearest the food box and thus formed the contact, but this method apparently affected neither the rapidity of each trial nor the number of days required for perfect learning. One of the normal rats placed his nose between the electrical contacts and received a shock, but other than one squeal and a vigorous rubbing of the nose, he showed no evidence of harm and had apparently forgotten the experience the following day. Some of the rats jumped to the point of operation from a distance; some placed the fore paws on the end of the plane and pressed down; and still others ran slowly around to the plane, halting an instant on the point of operation, and then continued the run around to the door. As a rule, the last made the best time. As in the maze experiment, many of the inbred rats were subject to errors which persisted throughout the experiment. In particular may be mentioned one rat that invariably formed a loop in the course from the entrance to the point of operation.

The shortest period of time required by an inbred rat to learn the inclined plane perfectly was twelve days; by a normal control rat, nine days. Eleven inbred rats and one control failed to learn the inclined plane within the one hundred days allowed.

In Table V is presented a comparative summary consisting of the daily averages of the entire inbred group and, directly beneath, the corresponding daily averages of the entire normal control group. The inbred rats required, on the average, 73.70+ days to learn the inclined plane; the controls but 45.97+ days. The absolute retention of the inbreds was, on the average, 31.842 seconds; of the controls, but 22.587 seconds. All

the inbreds had relearned at the end of the twenty-fourth day; but all the controls had relearned at the end of the seventeenth day. The inbred rats required, on the average, 6.74 days to relearn; the controls but 4.68 days.

In all these criteria of ability, learning, absolute retention and relearning, the rats of the normal control series are shown, on the average, to be superior to those of the inbred series.

The body length of the inbred rats used in the inclined plane experiment is, on the average, a trifle greater than that of the controls; the body weight is slightly less. The average actual brain weight of the inbreds is less than that of the controls. The relative brain weight (in reference to body length) of the inbreds is 5.89% less than that of the controls. The relative brain weight (in reference to body weight) of the inbreds is 2.38% less than that of the controls. Although the inbred rats were killed, on the average, at a more advanced age than the normal controls, the percentage of water in brain and cord is higher.

The figures presented in Table V support the hypothesis that a less than normal average brain weight in a strain of rats is accompanied by an average lesser ability to form habits.

TABLE Va

THE INCLINED PLANE

DAILY LEARNING AVERAGES OF INBRED AND NORMAL CONTROL RATS

(Time in seconds)

	Day 1	Day 2	Day 3	Day 4	Day 5
Inbred Average	4673.131	1218.976	166.997	56.576	22.926
Control Average	2769.953	1072.722	133.287	61.600	23.995
	Day 6	Day 7	Day 8	Day 9	Day 10
Inbred Average	36.878	12.422	11.061	10.751	8.136
Control Average	25.874	13.478	11.704	11.280	6.961
	Day 11	Day 12	Day 13	Day 14	Day 15
Inbred Average	9.383	7.876	8.625	7.188	9.586
Control Average	6.759	6.858	6.347	7.045	6.383
	Day 16	Day 17	Day 18	Day 19	Day 20
Inbred Average	8.710	8.069	7.364	9.191	6.717
Control Average	5.069	5.400	6.158	5.376	5.352
	Day 21	Day 22	Day 23	Day 24	Day 25
Inbred Average	6.919	6.363	6.951	6.458	7.329
Control Average	5.284	4.378	5.173	5.280	5.025
	Day 26	Day 27	Day 28	Day 29	Day 30
Inbred Average	5.674	6.262	6.627	5.514	27.802
Control Average	4.978	5.139	5.276	4.302	4.303

TABLE Va—(Continued)

	Day 31	Day 32	Day 33	Day 34	Day 35
Inbred Average	7.440	6.707	6.416	6.775	6.957
Control Average	4.354	5.075	3.583	3.868	4.024
	Day 36	Day 37	Day 38	Day 39	Day 40
Inbred Average	6.957	5.334	5.689	5.479	5.289
Control Average	4.121	4.457	3.958	4.905	4.096
	Day 41	Day 42	Day 43	Day 44	Day 45
Inbred Average	4.898	4.938	5.093	4.762	4.553
Control Average	3.748	4.033	3.446	3.729	3.857
	Day 46	Day 47	Day 48	Day 49	Day 50
Inbred Average	4.011	5.076	3.831	4.991	5.105
Control Average	3.367	3.091	3.159	3.050	3.113
	Day 51	Day 52	Day 53	Day 54	Day 55
Inbred Average	4.762	4.408	4.693	5.006	4.191
Control Average	3.044	3.160	2.841	2.924	3.759
	Day 56	Day 57	Day 58	Day 59	Day 60
Inbred Average	4.072	3.916	5.909	6.235	4.953
Control Average	3.000	3.392	3.047	2.991	3.375
	Day 61	Day 62	Day 63	Day 64	Day 65
Inbred Average	4.172	5.026	4.437	3.317	3.741
Control Average	3.173	3.251	2.848	3.951	3.035
	Day 66	Day 67	Day 68	Day 69	Day 70
Inbred Average	4.160	3.955	5.858	4.806	4.260
Control Average	3.098	3.109	3.155	3.138	2.949
	Day 71	Day 72	Day 73	Day 74	Day 75
Inbred Average	4.383	3.869	3.461	3.713	3.649
Control Average	2.976	3.072	3.163	2.640	3.129
	Day 76	Day 77	Day 78	Day 79	Day 80
Inbred Average	3.726	3.669	3.646	4.043	3.648
Control Average	2.667	2.799	2.770	2.750	2.839
	Day 81	Day 82	Day 83	Day 84	Day 85
Inbred Average	3.359	3.465	3.558	3.322	3.471
Control Average	3.094	2.663	2.665	2.652	2.823
	Day 86	Day 87	Day 88	Day 89	Day 90
Inbred Average	3.729	3.388	3.686	3.639	4.170
Control Average	2.763	2.834	2.669	2.681	2.663
	Day 91	Day 92	Day 93	Day 94	Day 95
Inbred Average	3.501	3.393	4.124	3.463	3.457
Control Average	2.683	2.665	2.623	2.732	2.846
	Day 96	Day 97	Day 98	Day 99	Day 100
Inbred Average	2.999	3.460	3.604	2.982	3.510
Control Average	2.772	2.657	2.608	2.946	2.637

	Failed to Learn	Days Required to Learn
Inbred Average	11	73.70+
Control Average	2	45.97+

TABLE Vb

THE INCLINED PLANE

AVERAGE ABSOLUTE RETENTION OF INBRED AND NORMAL CONTROL RATS

	Absolute Retention After 60 Days' Rest
Inbred Average	31.842 seconds
Control Average	22.587 seconds

TABLE Vc

THE INCLINED PLANE

DAILY RELEARNING AVERAGES OF INBRED AND NORMAL CONTROL RATS

(Time in seconds)

	Day 1	Day 2	Day 3	Day 4	Day 5
Inbred Average	41.789	7.301	5.436	4.783	5.067
Control Average	22.598	6.198	4.279	3.985	5.021
	Day 6	Day 7	Day 8	Day 9	Day 10
Inbred Average	5.175	4.239	3.404	3.186	2.828
Control Average	3.057	3.569	3.598	3.021	3.293
	Day 11	Day 12	Day 13	Day 14	Day 15
Inbred Average	3.508	2.712	2.796	2.845	2.807
Control Average	2.929	3.676	3.200	2.633	2.664
	Day 16	Day 17	Day 18	Day 19	Day 20
Inbred Average	2.610	2.603	2.596	2.733	2.677
Control Average	2.776	2.586	2.586	2.586	2.586
	Day 21	Day 22	Day 23	Day 24	Day 25 to 50
Inbred Average	2.607	2.729	2.596	2.554	2.554
Control Average	2.586	2.586	2.586	2.586	2.586

	Failed to Relearn	Days Required to Relearn
Inbred Average	0	6.74
Control Average	0	4.68

TABLE Vd

THE INCLINED PLANE

ANATOMICAL DATA OF INBRED AND NORMAL CONTROL RATS

	Body Length in mm.	Body Weight in grms.	Brain Weight in grms.	Cord Weight in grms.	Water in Brain per cent,
Inbred Average	195.93	184.37	1.72083	.53787	78.363
Control Average	194.43	189.18	1.81840	.53922	78.319

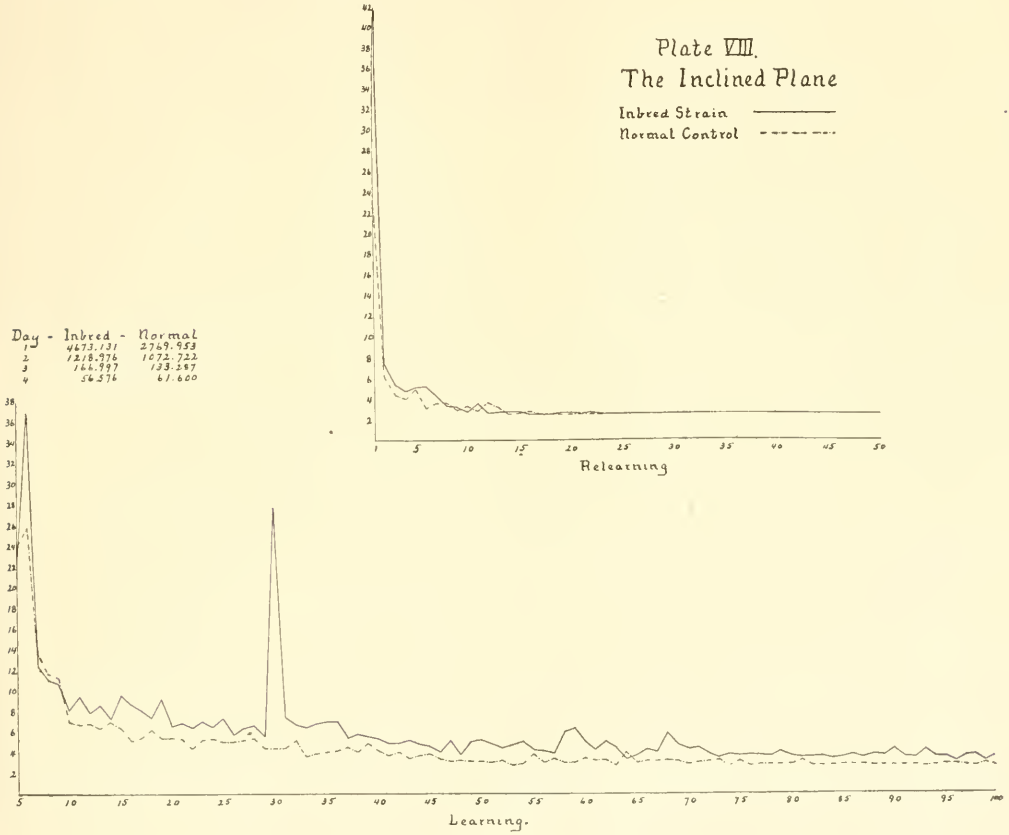
	Water in Cord, per cent	Per cent Brain Weight in Relation to Body Length	Per cent Brain Weight in Relation to Body Weight	Age killed, Days
Inbred Average	71.437	.87972	.97889	220
Control Average	71.223	.93474	1.00275	194

In Plate VIII is shown the curve of learning (below) and of relearning (above) of the inbred rats compared with those of the normal control. These curves are constructed from figures given in Table V. The curve of the inbred rats is indicated by the solid line, that of the normal control by the broken line. The ordinates give the average daily time in seconds for each group, and the abscissae the number of the day in which such time was made. As in the other learning curves, the time required by both inbred and control rats for the first four days was so long that it is represented here by figures and does not appear in the curve. The descent in time of both inbred and control rats for the first ten days is quite rapid, although both show retardation on the sixth day. From the forty-first day the curve of the controls lies entirely below the four second mark. The inbred curve, throughout, shows great irregularities, especially on the thirtieth day, when it rises to an average of nearly twenty-eight seconds. The inbred curve of relearning is very similar to that of the control, and from the twenty-third day coincides with it. But again, in relearning, we are dealing with selected groups, the eleven inbreds and two controls that failed to learn not being included. The inbreds of this selected group had all relearned at the end of the twenty-fourth day; the control at the end of the seventeenth day.

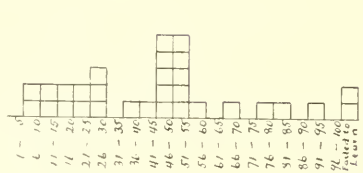
In Plate IX may be seen the distribution curves of learning of both the inbred and control series for the inclined plane experiment. The time is given in days—in groups of five for learning, singly for relearning. It is very apparent that the advantage lies wholly in favor of the normal control series.

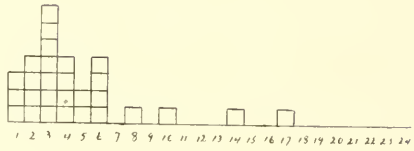
Of the inbred rats used in the inclined plane experiment, fifteen were from the seventh generation, fourteen from the eighth, and one from the ninth. In Table VI is presented a comparative summary consisting of the daily averages of the seventh and eighth generation rats used in the inclined plane experiment. With the rats of the eighth generation may be included the one from the ninth, as her record was not far from the average of the eighth. The table shows that four of the seventh generation and seven of the eighth generation failed to learn the inclined plane. The seventh generation required, on the average, 59.60+ days to learn; the eighth generation, 86.53+ days. The absolute retention of the seventh genera-

tion was, on the average, 44.945 seconds; of the eighth generation, 13.825 seconds. All the seventh generation had relearned at the end of the twenty-fourth day; but all the eighth generation had relearned at the end of the eighth day. The seventh generation required, on the average, 8.00 days to relearn; the eighth generation but 5.00 days.



In these criteria of ability the seventh generation excelled in learning, the eighth in absolute retention and relearning. But, again, in absolute retention and relearning, we are dealing with selected groups as the seven eighth generation and four seventh generation rats that failed to learn were not used. There seems, on the whole, to be but little difference between the abilities of





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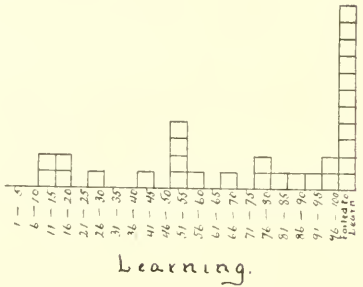
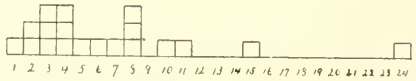




Inbred.

Plate IX.

The Inclined Plane

Distribution of Learning and Relearning

the seventh and eighth generations except that the former excelled in learning.

The body length and body weight of the seventh generation average greater than those of the eighth. The relative brain weight (in reference to body length) of the seventh generation is 5.20% less than that of the eighth. The relative brain weight (in reference to body weight) of the seventh generation is 13.93% less than that of the eighth. The actual brain weight of the seventh generation, however, is greater than that of the eighth. The percentage of water in brain and cord of the seventh generation is greater than in the eighth.

TABLE VI

THE INCLINED PLANE

DAILY LEARNING AVERAGES OF SEVENTH AND EIGHTH GENERATION INBRED RATS

(Time in seconds)

	Day 1	Day 2	Day 3	Day 4	Day 5
Seventh Average	5656.546	1463.707	123.213	53.583	17.159
Eighth Average	3689.716	974.245	210.781	58.369	28.693
	Day 6	Day 7	Day 8	Day 9	Day 10
Seventh Average	11.142	11.373	8.160	8.845	5.853
Eighth Average	62.613	13.471	13.961	12.657	10.419
	Day 11	Day 12	Day 13	Day 14	Day 15
Seventh Average	10.855	5.459	6.037	5.197	6.367
Eighth Average	7.911	10.294	11.213	9.178	12.805
	Day 16	Day 17	Day 18	Day 19	Day 20
Seventh Average	5.821	4.732	6.714	6.351	5.310
Eighth Average	11.599	11.407	8.013	12.030	8.124
	Day 21	Day 22	Day 23	Day 24	Day 25
Seventh Average	5.592	5.538	4.934	6.116	5.481
Eighth Average	8.245	7.187	8.969	6.801	9.177
	Day 26	Day 27	Day 28	Day 29	Day 30
Seventh Average	6.204	5.636	5.809	3.965	4.875
Eighth Average	5.143	6.889	7.445	7.063	50.729
	Day 31	Day 32	Day 33	Day 34	Day 35
Seventh Average	4.085	5.884	4.733	4.195	4.526
Eighth Average	10.795	7.529	8.099	9.355	9.388
	Day 36	Day 37	Day 38	Day 39	Day 40
Seventh Average	4.481	3.921	4.192	4.467	4.561
Eighth Average	9.433	6.747	7.186	6.491	6.017
	Day 41	Day 42	Day 43	Day 44	Day 45
Seventh Average	4.667	4.462	4.396	4.338	3.734
Eighth Average	5.130	5.413	5.791	5.187	5.373

TABLE VI—(Continued

	Day 46	Day 47	Day 48	Day 49	Day 50
Seventh Average	3.435	4.121	3.636	3.867	4.277
Eighth Average	4.587	6.031	4.027	6.116	5.933
	Day 51	Day 52	Day 53	Day 54	Day 55
Seventh Average	4.289	3.591	4.219	4.969	3.627
Eighth Average	5.235	5.226	5.168	5.044	4.755
	Day 56	Day 57	Day 58	Day 59	Day 60
Seventh Average	3.251	3.584	4.406	7.917	4.325
Eighth Average	4.893	4.247	7.412	4.553	5.581
	Day 61	Day 62	Day 63	Day 64	Day 65
Seventh Average	4.295	5.753	4.731	3.006	3.331
Eighth Average	4.049	4.299	4.143	3.627	4.151
	Day 66	Day 67	Day 68	Day 69	Day 70
Seventh Average	3.331	3.571	4.317	5.175	4.565
Eighth Average	4.988	4.339	7.398	4.437	3.954
	Day 71	Day 72	Day 73	Day 74	Day 75
Seventh Average	4.415	3.717	3.388	3.233	3.620
Eighth Average	4.350	4.021	3.533	4.193	3.678
	Day 76	Day 77	Day 78	Day 79	Day 80
Seventh Average	3.760	3.601	4.130	4.705	3.623
Eighth Average	3.691	3.736	3.162	3.381	3.673
	Day 81	Day 82	Day 83	Day 84	Day 85
Seventh Average	3.319	3.039	3.322	3.135	3.424
Eighth Average	3.398	3.891	3.794	3.509	3.519
	Day 86	Day 87	Day 88	Day 89	Day 90
Seventh Average	3.251	3.037	3.144	3.361	3.837
Eighth Average	4.207	3.739	4.228	3.916	4.503
	Day 91	Day 92	Day 93	Day 94	Day 95
Seventh Average	3.211	3.157	3.273	2.913	2.678
Eighth Average	3.792	3.629	4.975	4.014	4.237
	Day 96	Day 97	Day 98	Day 99	Day 100
Seventh Average	2.725	2.650	3.085	3.201	3.067
Eighth Average	3.273	4.270	4.123	2.763	3.954

	Failed to Learn	Days Required to Learn
Seventh Average	4	59.60+
Eighth Average	7	86.53+

TABLE VIb

THE INCLINED PLANE

AVERAGE ABSOLUTE RETENTION OF SEVENTH AND EIGHTH GENERATION INBRED RATS

	Absolute Retention After 60 Days' Rest
Seventh Average	44.945 seconds
Eighth Average	13.825 seconds

TABLE VIc

THE INCLINED PLANE

DAILY RELEARNING AVERAGES OF SEVENTH AND EIGHTH GENERATION INBRED RATS

(Time in seconds)

	Day 1	Day 2	Day 3	Day 4	Day 5
Seventh Average	66.322	9.265	6.322	5.758	6.279
Eighth Average	8.058	4.438	4.218	3.443	3.400
	Day 6	Day 7	Day 8	Day 9	Day 10
Seventh Average	6.297	4.467	5.879	3.728	3.109
Eighth Average	3.633	3.925	2.441	2.441	2.441
	Day 11	Day 12	Day 13	Day 14	Day 15
Seventh Average	4.285	2.909	3.055	3.139	3.073
Eighth Average	2.441	2.441	2.441	2.441	2.441
	Day 16	Day 17	Day 18	Day 19	Day 20
Seventh Average	2.733	2.721	2.709	2.945	2.848
Eighth Average	2.441	2.441	2.441	2.441	2.441
	Day 21	Day 22	Day 23	Day 24	Day 25 to 50
Seventh Average	2.727	2.939	2.709	2.636	2.636
Eighth Average	2.441	2.441	2.441	2.441	2.441

	Failed to Relearn	Days Required to Relearn
Seventh Average	0	8.00
Eighth Average	0	5.00

TABLE VId

THE INCLINED PLANE

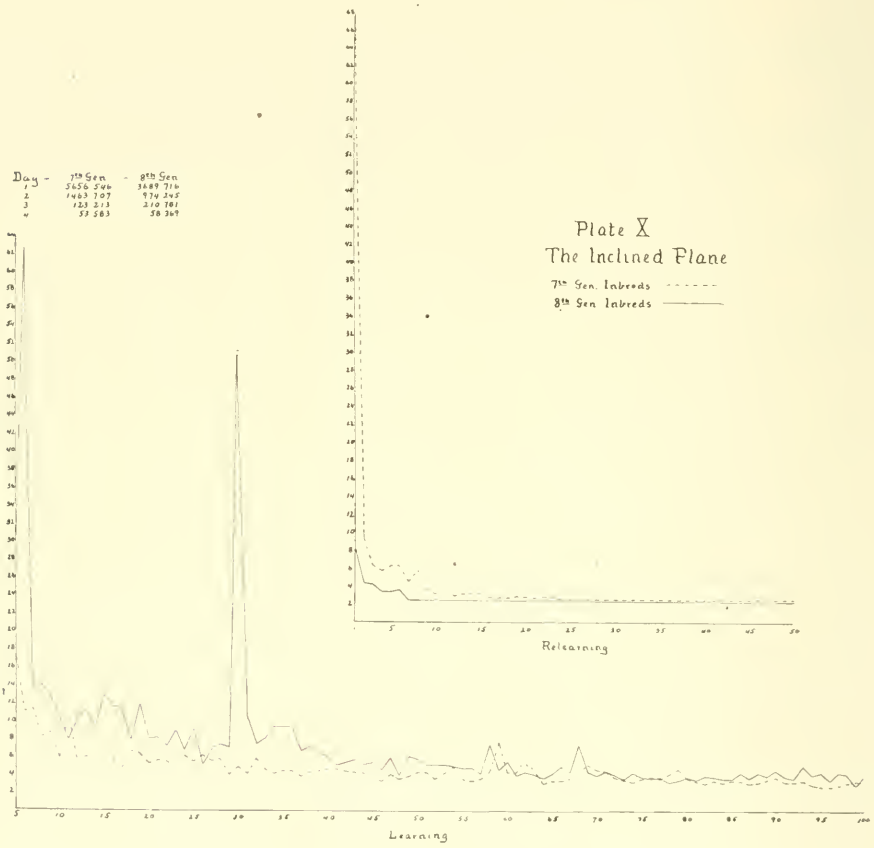
ANATOMICAL DATA OF SEVENTH AND EIGHTH GENERATION INBRED RATS

	Body Length in mm.	Body Weight in grms.	Brain Weight in grms.	Cord Weight in grms.	Water in Brain, per cent
Seventh Average	202.13	201.44	1.72868	.54607	78.542
Eighth Average	189.73	167.33	1.71299	.52967	78.185

	Water in Cord, per cent	Per cent Brain Weight in Relation to Body Length	Per cent Brain Weight in Relation to Body Weight	Age killed, Days
Seventh Average	71.569	.85622	.90560	223
Eighth Average	71.304	.90323	1.05218	217

In Plate X is shown the curve of learning (below) and of relearning (above) of the seventh generation of inbred rats compared with those of the eighth. These curves are constructed from figures given in Table VI. The curve of the seventh generation rats is indicated by the broken line, that of the eighth generation

by the solid line. The ordinates show the average daily time in seconds for each group, and the abscissae the number of the day in which such time was made. Both curves in the learning series are very irregular, especially so that of the eighth generation rats. Although irregular, the seventh generation curve lies



below that of the eighth except in a few instances. From the first, the relearning curves are similar and very regular, although the eighth generation curve remains below that of the seventh all the way. But both of the relearning groups are selected, four of the seventh and seven of the eighth generation having failed to learn the inclined plane.

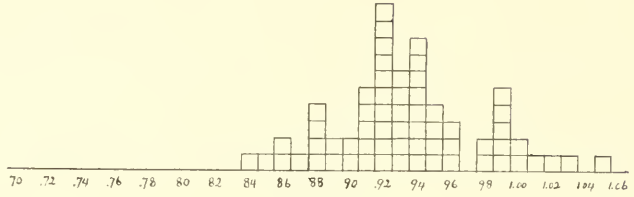
VI. SUMMARY AND CONCLUSIONS

During a series of experiments in inbreeding conducted at the Wistar Institute of Anatomy and Biology, a strain of albino rats was produced, the relative brain weights of which averaged considerably less than normal. Whether such a condition was induced by the inbreeding or was due to environmental factors can not be stated with certitude at the present time. Inbreeding, *per se*, may not be, necessarily, productive of deleterious results if the parent stock be perfect in every respect; but it is impossible, by any means at our command, to determine physical perfection in any organism. An environmental factor that may have had some bearing on the lesser relative brain weight condition of the two strains of rats (A and B) used in these experiments was, that after four generations of inbreeding the rats did not appear to thrive; at that time a change of diet took place, after which they seemed in better health.

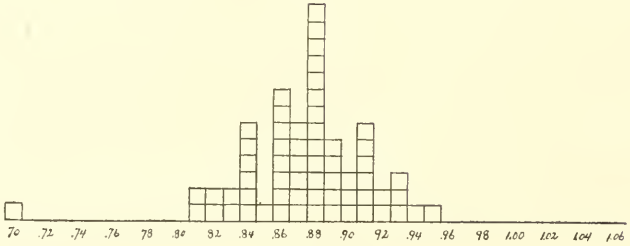
The writer spent two years in the task of attempting to ascertain whether or not the less than normal relative brain weight was accompanied by a corresponding lesser ability to form habits, and, also, if such ability was progressively less from one generation of inbreeding to the next. There were used in all the experiments one hundred and twenty-four rats: sixty-two inbreds and sixty-two normal controls. An equal number of males and females from inbreds and controls were used in each experiment. Plate XI shows the distribution of relative brain weights (with reference to body length) of the inbred rats and of the normal control series. The inbred distribution is represented by the lower curve, that of the normal control by the upper. The greatest frequency in the inbred curve occurs at .88%; in the normal curve at .92%. The entire inbred distribution is from .70% to .95%; that of the normal controls from .84% to 1.05%. The average relative brain weight (with reference to body length) of the sixty-two normal control rats is .93351%; that of the inbreds is .87335%, or 6.44% less than that of the normal control.

In order to compare the ability of the rats of the lesser brain weight strain (inbred rats) with a normal control series, three experiments were used:

1. The Maze, in which all the rats used were given five trials daily until they had learned perfectly, or, failing to learn, had



Normal



Inbred.

Plate XI

Distribution of Relative Brain Weights
(with reference to body length)

62 Inbred Rats.
62 Normal Rats.

worked one hundred days (500 trials). At the expiration of sixty days after perfect learning the rats, except those failing to learn, were tested for absolute retention and relearning until relearning was perfect, or, failing relearning, for fifty days (250 trials).

2. The Preliminary Inclined Plane, in which all the rats used were given five trials daily for twenty days (100 trials); at the expiration of sixty days after this period they were all tested for absolute retention and relearning for a period of five days (25 trials).

3. The Inclined Plane, in which all the rats used were given three trials daily until they had learned perfectly, or, failing to learn, had worked one hundred days (300 trials). At the expiration of sixty days after perfect learning the rats, except those failing to learn, were tested for absolute retention and relearning until relearning was perfect.

In all these experiments the strain of rats of lesser relative brain weight (the inbreds) learned less well, on the average, than the normal control series. In the maze and inclined plane experiments the average number of days required to learn and relearn, and the time of absolute retention, was far greater in the case of the inbred rats than in that of the normal control series. In the maze experiment, two inbreds and one control failed to learn; two inbreds failed to relearn. In the inclined plane experiment, eleven inbreds and two controls failed to learn.

The similarity of behavior of the control rats containing blood of the B strain to that of the inbreds suggests the importance of crossing a strain of inbred rats of lesser brain weight with normal rats, and carrying out a series of tests such as have been presented in this paper, with two controls: one of normal rats, and one of rats of lesser relative brain weight.

In the maze experiment the inbred rats of the seventh generation did a little less well than those of the sixth. In the inclined plane experiment the rats of the eighth generation did a little less well than those of the seventh. It would seem (although lessening of relative brain weight had ceased after the fourth generation of inbreds) that the ability to form habits lessened progressively with successive generations of inbreeding.

The writer had intended to attempt a correlation (if any existed) between the number of days required to learn a habit

and the number of days required to relearn after sixty days' rest. But most of the rats relearned very quickly without reference to the number of days required for learning; in numbers, too, the rats were too few for such mathematical consideration. An investigation along such a line should consist of but one relatively simple experiment; several hundred rats of one sex only should be used; and the period of time between the completion of learning and the beginning of relearning should be lengthened to, at the least, ninety days.

The general results of the experiments here set forth may be summed up as follows: On the average, the strain of inbred rats having a less than normal relative brain weight did less well in learning to form habits than did the normal control series.

From these results the following may be formulated: *A less than normal brain weight in a strain of rats is accompanied by a less than normal ability to form habits.*

ADDENDUM

The tables of individual daily averages, from which the tables of group averages contained in this monograph are derived, are so extensive as to preclude publication. If, however, any may be interested in them, the original copy is deposited with the library of the Wistar Institute of Anatomy and Biology, Philadelphia, Pennsylvania; and a duplicate is in the private library of the author.

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BEHAVIOR MONOGRAPHS

Volume 2, Number 5, 1915 Serial Number 10

Edited by JOHN B. WATSON
The Johns Hopkins University

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CONTENTS

		Page
I.	Apparatus and Method when a Single Problem is Learned	2
	Results with the Latch Box	7
	Results with the Circular Maze	15
II.	The Effect of Different Distributions of Practice when Three Problems are Learned Abreast	23
	Results Showing Effect of Distribution of Practice	26
	Summary of Experiments on Learning	41
III.	Retention	43
	Discussion of Results	46
	Some General Remarks on the Integration of Movements	49
	Conclusions	50

DISTRIBUTION OF EFFORT IN LEARNING IN THE WHITE RAT[1]

JOHN LINCK ULRICH

The present study of the distribution of effort in learning was begun with white rats as subjects in the Psychological Laboratory of the Johns Hopkins University in the fall of 1911, and was completed in the spring of 1913. The questions which the investigation attempts to answer are as follows: (1) When the rat learns a simple problem of manipulation, what is the most efficient method, from a physiological standpoint, of giving the trials? From the standpoint of rapidity of learning, minimization of excess effort and length and steadfastness of retention, shall we allow the animal to work at the given problem once per day, three times per day, or five times per day, etc.? (2) Having answered this in connection with the learning of a single problem, the same question comes up again when it is asked whether the relations found to hold good in (1) will apply to cases where the animals have to learn more than one problem at a time. In other words, we shall attempt to find out whether in learning three problems abreast it is more advantageous to give one trial per day on each of the three problems, three trials per day on each of the three problems, or five trials per day on each of the three problems. (3) Since in carrying out the above experiments we shall have a given problem box learned under two different sets of conditions, viz., (a) alone, (b) concurrently with two other problems, we shall have the data necessary for answering the question whether it is methodologically more efficient (i.e., requires fewer trials) to require the animals to learn (under the several types of distribution of practice) one problem completely before taking up a second one or whether better results may be obtained by requiring them to learn the several problems abreast. (4) The final subject dealt with in this paper attempts to show the effect upon retention of allowing the animals to learn by the one trial, three, five, etc., trials methods.

[1] From the Psychological Laboratory of the Johns Hopkins University.

The effect of distribution of practise on learning has been studied by Ebbinghaus (1) and extensively by Jost (2), also to some extent by Leuba and Hyde (3), Munn (4), Starch (5), Pyle (6, 7), Kirby (8) and Whitley (9). The subjects in all of these cases were human beings, and the material was made up of nonsense syllables, substitution of German script in writing English words, letter substitution, mental arithmetic, etc. A complete summary of this work is given by Thorndike.[2] Pyle's conclusions are of especial interest in this connection. Working with a substitution test he concludes: "On the whole, 30 minutes seems to be the best length of practice period. In some cases, shorter periods seem a trifle more advantageous, especially in the early stages of practice or habituation. But, generally speaking, one gets ample returns in habituation for practicing up to the point of fatigue, which in our experiments proves to be 30 or 40 minutes for most subjects. Eighty minutes, the longest period used, proved decidedly disadvantageous, especially in the early stages of habituation. Generally speaking, daily practice seems to give better returns than the same number of periods distributed on alternate days or in twice-a-day periods. However, there is some evidence that in the early stages of habituation, the second practice on the same day gives good returns and that, later on, alternate days may be the best distribution." The small amount of work which has been done upon the distribution of effort in learning in animals has been summarized by Watson (10). Tests upon the dancing mouse by Yerkes (11) indicate that for the white-black habit the fewer the tests per day within the limits of two and one hundred, the higher the efficiency of the method of training as measured in terms of the total number of tests necessary for the establishment of a perfect habit, and the lower its efficiency as measured in terms of the number of series given.

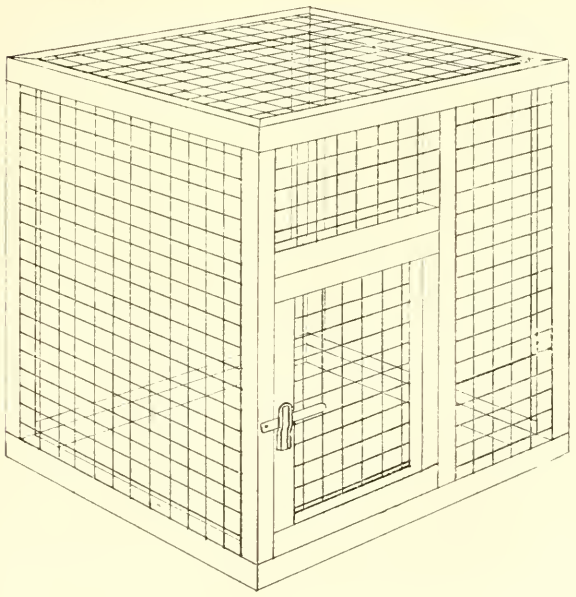
I. APPARATUS AND METHOD WHEN A SINGLE PROBLEM IS LEARNED

The apparatus used for testing the effect of the various distributions of practise in learning a single problem comprised the latch box as shown in Fig. I, and the circular maze described on p. 16. The latch box was about 28 cm. square. In the

[2] Thorndike, E. L. Educational Psychology, vol. II, p. 195 ff.

left hand corner of one side of the box was a door 15 cm. by 18 cm. The box as well as the door was covered with wire netting of 1.3 cm. mesh. A wooden latch attached to the frame of the box fitted in a notch in the door. Inside the door there was a light spiral spring which opened the door when the latch was released and it also held the latch more firmly in the notch. The hood that covered the latch box had a base 50 cm. square and 43 cm. high. It was hinged to the top of a table to prevent it shifting out of position. Opposite the hinged side, and about 25 cm. from the door of the latch box, was the entrance of the hood. An entrance box of wire netting admitted the rat to the hood. The latch box remained always in the same position on the table under the hood.

FIG. 1
LATCH BOX.



This box has been used before in animal work in a similar or modified form. All that was necessary for the rat to do to solve the problem was to open the door of the box. The rats usually opened the box by raising the latch with their snouts. The problem was not a difficult one to solve, for during learning, movements were made that were often far more complex than were necessary to open the door.

Young rats were used for experimentation. Usually, when they were thirty-five days old, the mother and her litter were separated. The young were then old enough to be self-dependent. When forty to forty-five days old the rats were divided into groups of two or four, the sexes being separated. At this age their activities showed a ready adroitness that older animals do not display; they were excitable and curious—characteristics which are admirable for experimentation. No pronounced fixed habits had likely been learned, and the simple environment of the cage had not become monotonous, as was shown by the fact that they indulged more rarely in sleep during the day than the adults.

The incentive to all activity in experimentation was food. When it is used as a stimulus two considerations are present that require attention in order to obtain the best results, viz., the kind of food, and the amount of food. The usual food was a preparation of bread and milk, and in addition occasional pieces of fruit, carrot, and grain were added to maintain that metabolic equilibrium which a variety of food produces and which is so necessary to a healthy organism. Since the amount of food also is quite as necessary a consideration, in view of the fact that the activity of the rat is largely dependent upon this factor, particular attention was as a consequence paid to its control. It was early apparent how needful active metabolism was to the production of responses which would secure success in learning. Activity, conditioned by the need for food, is in the nature of a normal life process.

As far as practicable experimentation was done in the morning hours, though when several groups of rats were under consideration the work extended into the afternoon. No time was lost after the operator entered the room before actual work was begun. Food was prepared for the rats, and experimentation was usually continued without interruption. The reason for

this was because it was early noticed how eager the rats were to feed when the operator entered the room for the first time, and how this eagerness subsided after he had been present for a time. This association of the operator with the securing of food was seized upon as an advantageous time for work. Of course, when there were from five to ten groups of rats under observation, it was impossible to reach the last of them before this eagerness was dissipated. This was partly overcome by instituting a system of rotation in handling the groups, whereby a different set was taken first each morning.

In the beginning of the work on this problem it was thought necessary, when taking rats at forty days old, to give a few preliminary runs in the Porter maze before actual experimentation. This was done to familiarize young rats with strange surroundings, and to accustom them to being handled. This early preparation was continued in all the work with the latch box when used alone, but abandoned as unnecessary with the other problems, the circular maze and the inclined plane box. It was discovered that the rats were quite ready to solve these problems without such preparation. They were active at all times and not unduly disturbed by the change from the environment of the cage. After the preliminary runs in the maze, the rats were fed for two days in the problem box, after which they were for the first time given the problem to solve.

At first an entrance box was taken to the cage in which the rats were confined, and then a transfer was made to the hood, but this method was soon abandoned, as it was found exceedingly difficult to tame the rats without some handling. An untrained and naturally excitable rat became greatly disturbed when carried in a closed cage, and it usually required several minutes before it became calm, and, of course, such disturbances could produce nothing but undesirable results. A new procedure was, therefore, adopted. Rats were carried by hand to the entrance box of the hood and then admitted. At no time were they petted or coddled. In fact they were handled as little as possible. When a rat was greatly excited or very timid, it was left in the entrance box until it became calm before it was permitted to enter the hood.

One group of 17 rats was given one trial, another group of 11 three trials, and a third group of 11 five trials daily. Rats

having had one trial were permitted to feed a short time in the problem box, and were then removed and fed in their cages. Those given three and five trials were allowed, after each trial, to nibble a little food. At the end of all trials, these rats, like those given one trial, were fed in their cages. No harm seemed to have resulted from not feeding the rats entirely in the problem box after experimentation was over; on the other hand, there were material advantages gained by not doing so. Time was saved in experimentation, and a variety of food was given which would have been impossible if the rats were fed only in the problem box.

The time for each trial was taken by a stop watch in minutes, seconds and half seconds. Later the seconds were converted into decimals of a minute. Time spent by the rats nosing about the entrance box when it was opened was not counted, but only from the moment the rat entered the hood covering the latch box until the problem was solved.

Daily experimentation was continued until a rat reached a norm, or an average of one second per trial and held it for two successive days. This norm was required of all rats regardless of the number of trials which they were given each day. In other words, in the last two days of experimentation, a rat had to attain an average of one second in two trials with one trial daily, an average of one second in six trials with three trials daily, and an average of one second in ten trials with five trials daily. When an animal lifted the latch of the latch box in an unusual way, or habitually went around the box before attacking the latch, the time was increased considerably, and in these rare cases a norm of two seconds was taken.

The usual norm of one second was not a difficult one to reach, for the more active rats learned how to lift the latch in one-half of a second. Such quick time as one-half of a second was more often attained by a rat given one trial than those given three or five trials daily. When either norm was reached, learning was assumed to be complete.

After completion of learning the rats were held for retention. The retention tests were taken after an interval of sixty days. In the very beginning of this work, the rats were tested after thirty days, but retention was so perfect at the end of this time that the interval was doubled. During the sixty days of no

practice the rats were not left idle in their cages, but were permitted to exercise once a day in the maze when feeding. It was the only method at hand to avoid the evils that arise from close confinement, of which there are a great many. The most serious one is the rapid increase in weight that usually occurs after a period of experimentation. A rat, having been once restricted as regards food, has a tendency to overfeed and to take on fat. Several days before the retention tests were made the amount of food was still more restricted, and conditions similar to those maintained when learning was in progress were established.

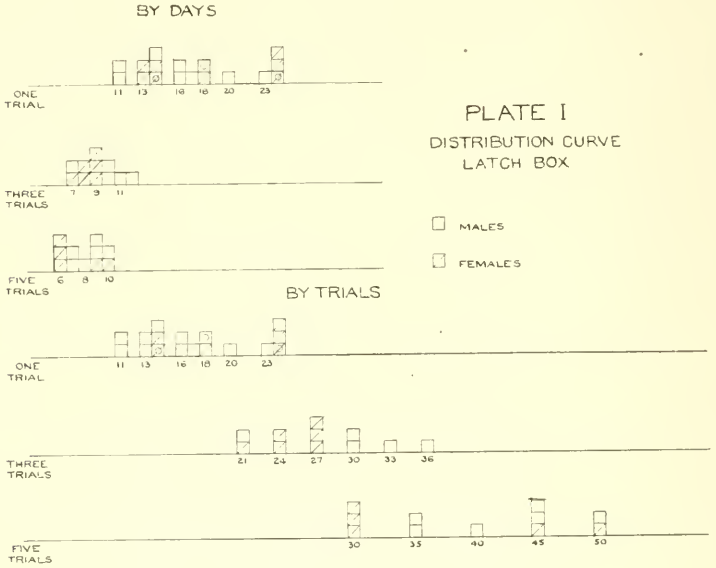
RESULTS WITH THE LATCH-BOX

As a rule, as soon as the rats entered the hood, they made directly for the door of the latch box, the more timid approaching cautiously. Having been fed previously for two days in the problem box, and finding their way obstructed by the closed door, they usually ran around and over the problem box and climbed inside the hood, but eventually attacked the door. Their attack was often violent, and all methods imaginable were tried before success was attained. As a result, the first solution of the problem was made in complex and in unique ways. But these ways were soon to disappear, and with them the sharpness and the suddenness of action which appeared in the movements of the early trials. With their disappearance a noticeable modification occurred, movements became more precise, indicating that integration was in progress.

To print the results of the daily records of each rat is impossible and accordingly only the average results are here presented. These are given by distribution curves and by tables. The former are arranged to give the number of trials and the number of days each rat required to learn the problem and their distribution; the latter, the averages for each trial for all rats in the one, three, and five trial groups. From the tables ordinary curves are plotted to show the process of the integration of the movements. Interesting as the records of each rat would be, the important data, the averages, will show sufficiently well the more important results of the experiment.

The number of trials, or the total time consumed in learning, and the distribution in the number of days before each rat mastered the problem are indicated on the distribution curve,

Plate I. Both the number of trials and the number of days required are shown by squares arranged on base lines. Males are indicated by plain squares, and females by squares with a diagonal. The squares on the base lines on the lower half of the plate show the number of trials given each individual rat in the one, three and five trial groups. The upper half of the plate shows the number of days required, and their distribution. The rats which required a norm higher than one second are indicated by a circle in the squares.



On a close examination of this plate, it is seen that a group of rats given one trial daily learned the problem with fewer trials than those given three or five trials. This is shown best by noting in how many trials the first rat in each of the groups completed the task. The first rat in the one trial group learned the problem in 11 trials, in the three and five trial groups in 21 and 30 trials respectively. A like comparison can be made of the three groups with rats that required the greatest number of trials. These results show that learning is most economical (number of trials) when one trial is demanded of a rat. But, when considering the number of days demanded for the prob-

lem, three and five trials daily are more economical than is one trial daily. In this case, the first rat of the one trial group required 11 days, of the three and five trial groups 7 and 6 days. From these results it is seen that when trials are distributed

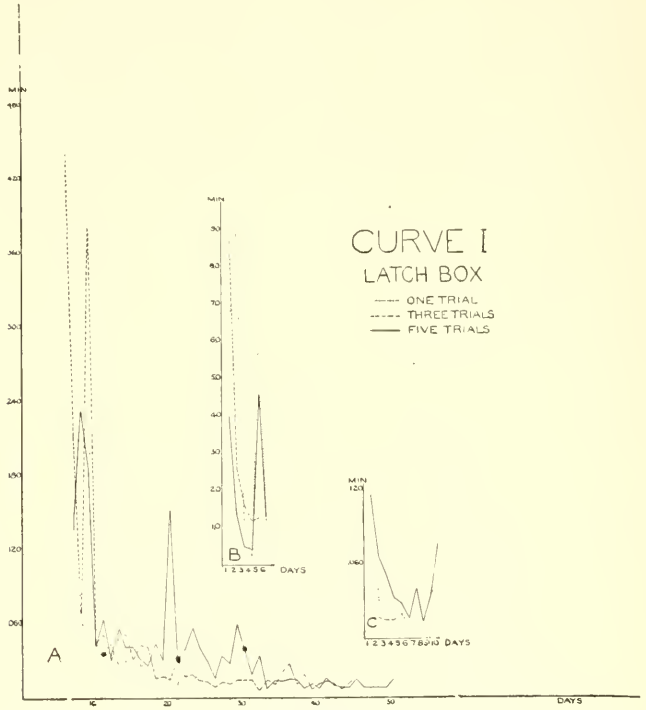
TABLE I

LATCH-BOX

Trial No.	One trial average	Trial No.	Three trials average	Trial No.	Five trials average	Trial No.	One trial average	Trial No.	Three trials average	Trial No.	Five trials average
1	6.37	1	9.22	1	4.30			34	.012	34	.014
2	8.99	2	2.61	2	1.35			35	.020	35	.015
3	1.16	3	1.61	3	.517			36	.029	36	.016
4	.244	4	1.09	4	.505			37	.012	37	.010
5	.574	5	1.26	5	4.670			38	.020	38	.011
6	.601	6	.440	6	1.090			39	.008	39	.014
7	.085	7	.212	7	.136			40	.012	40	.011
8	.059	8	.069	8	.233			41	.016	41	.016
9	.064	9	.051	9	.187			42	.012	42	.014
10	.044	10	.044	10	.200			43	.008	43	.010
11	.036	11	.050	11	.063			44	.012	44	.010
12	.045	12	.032	12	.036			45	.018	45	.016
13	.028	13	.049	13	.057					46	.010
14	.032	14	.052	14	.041					47	.010
15	.044	15	.033	15	.042					48	.010
16	.025	16	.043	16	.031			Retention		49	.010
17	.031	17	.042	17	.028					50	.017
18	.034	18	.017	18	.044			1	.021		
19	.026	19	.018	19	.029			2	.017		
20	.021	20	.016	20	.148			3	.015		
21	.013	21	.035	21	.031			4	.015	Retention	
22	.017	22	.019	22	.038			5	.009		
23	.015	23	.020	23	.057			6	.010	1	.117
24	.019	24	.016	24	.044					2	.066
		25	.015	25	.030					3	.046
		26	.013	26	.016					4	.033
		27	.015	27	.035					5	.025
Retention		28	.012	28	.029					6	.016
		29	.015	29	.060					7	.040
1	.097	30	.016	30	.041					8	.013
2	.030	31	.016	31	.020					9	.028
		32	.009	32	.035					10	.079
		33	.014	33	.011						

over several days fewer trials are required to learn a problem, and the accumulation of a number of trials in one day resulted in the need of fewer days to perform the task. A discussion of the significance of these results will be reserved until p. 46.

The averages for each trial for all rats, both males and females, are given in Table I. These averages show that there is in succeeding trials a gradual decrease in time for each trial while learning is in progress. This decrease, when plotted in a curve, indicates the progressive steps in learning. Curve I, of the latch box, gives the individual curves of the one, three and five trial groups.



Before attempting any analysis of these curves, it is well, at this stage, to describe the structure of all the curves of this kind. A division of each individual curve was found necessary because of the great length of all of the curves, and it was also desirable to indicate the daily variations in time for the solution of the problem toward the end of the learning period. To do this, the first part of the curves was constructed in tenths of a minute and the latter part of the curve in thousandths of a minute. All the curves were plotted in the same manner. The

points on the abscissa represent trials, and those on the ordinate the average time of all rats in a given group at any given trial. A length of $\frac{1}{2}$ mm. on the abscissa represents one trial, and $\frac{1}{2}$ mm. on the ordinate represents for curve A and C .006 of a minute, and for curve B 0.2 of a minute. The early trials are plotted in curve A and the later trials in curve B. Curve C is the one for retention tests.

In general, these curves give the best available indication of the entire progress of learning. When critically considered, they show not only the changes in the physiological state of the organism during learning, but they also show the physiological changes resulting from extraneous disturbances. Of course, the latter are antagonistic to right reactions and integration. How to distinguish one kind from the other will ever be a difficult matter. In reading all curves of this kind, it is always necessary to assume the presence of harmful disturbances.

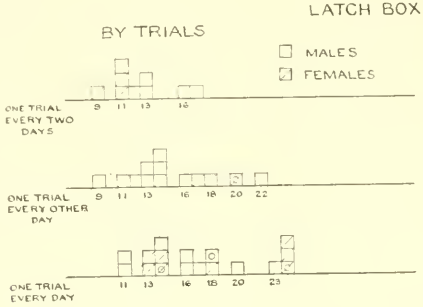
Having described the construction of the curves it will now be convenient to mention the characteristics of the curves of learning. Consider, for example, Curve I, above. The most important features are the great length, and the irregularity of curves representing the three and five trial groups as compared with the one trial group. The individual curve of the five trial group shows, because of the greater number of trials necessary to learn the problem, the greatest length. The shortest of the curves is that of the one trial group. Moreover an indication of the greater lengths of curves of the groups given several trials daily is evident in that part of the curves before the rapid drop or descent is made. More trials are here required before the drop takes place than when only one trial is given. With the three and five trial groups this part of the curves drops in the twelfth trial, and that of the one trial in the eighth trial. And, in a similar manner, the remaining portions of the three curves show differences. As compared with the other two groups, that of the one trial is prolonged but a short distance beyond the drop. This part of the curve of the five trial group is the longest of the three. So, too, the irregularities in all parts of the curves, particularly in the early trials, are more prominent when several trials are presented than when one trial is given. In all probability this irregularity combined with the length of the curve is indicative

of the fact that greater effort was demanded of the rats in the three and five trial groups.

These curves of integration being constructed on the average time made by all rats, do not express individual differences in learning. Some rats naturally in this respect showed interesting differences in their capacity to learn the problem; the ends of the curves point to where the least adaptable rats left off learning. To indicate where the first rat of each group completed its task a small circle has been added to the curves. These two points, the ends of the curves and the position of the circles, give the extreme individual differences in the power of learning of the rats in this experiment.

PLATE II

DISTRIBUTION CURVE
LATCH BOX



The demonstrated fact that one trial daily was more economical than when several were demanded suggested further work with the latch box. It was thought interesting to determine whether fewer trials would be necessary to learn the problem if efforts were distributed over more days. A trial, therefore, was given one group of rats every other day, and another group every third day, that is, with two days intervening between trials.

This particular addition to the problem of the latch box was conducted under the same conditions and with the same strain of rats, the descendants of those previously worked with. A more satisfactory group of rats could hardly have been secured than those used in this experiment, since they were of an unusually hardy strain. On the intervening days, when the rats

were not being trained, they were allowed the freedom of the square maze during feeding time to keep down body weight and to prevent the accumulation of fat. A day or so between trials had a marked effect on the increase in weight, and the amount of food given had to be carefully watched. Whether this increase was due to periods of rest is difficult to say.

TABLE II

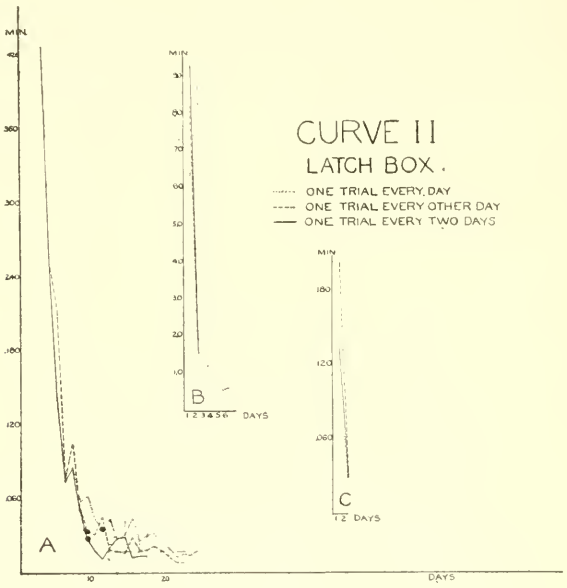
Latch Box

Trial No.	One Trial Every Other Day	Trial No.	One Trial Every Third Day
1	8.38	1	9.38
2	1.93	2	1.59
3	.663	3	.433
4	.250	4	.246
5	.215	5	.143
6	.078	6	.077
7	.107	7	.086
8	.053	8	.057
9	.035	9	.029
10	.032	10	.019
11	.046	11	.012
12	.019	12	.021
13	.018	13	.029
14	.017	14	.030
15	.030	15	.014
16	.019	16	.016
17	.019	17	.016
18	.022		
19	.019		
20	.014	Retention	
21	.016		
22	.016	1	.134
		2	.026
Retention			
1	.204		
2	.032		

The number of trials and days required to learn the problem and their distribution are found on Plate II. The data here presented are more striking than those previously reported. It shows that still fewer trials were required than with one trial daily, and a trial every third day necessitated the fewest trials.

It will be seen that the first rat completed learning with one trial every third day in 9 trials, with one trial every other day also in 9 trials, and with one trial every day in 11 trials. The last rats to finish in these experiments did so in 17, 22, and 24 trials respectively. On the whole fewer trials are necessary to learn a problem when trials or periods of learning are distributed over several days.

The curves, Curve II, below representing the progress of integration must still be described. They were plotted from the



averages of Table II. The individual curve of the one trial group is plotted again to show the difference between it and the curves representing the groups working every other day and every third day. These latter curves have the same general appearance and contour as the former (1 trial), though they show some essential differences. The curve for one trial daily is the longest and the most irregular, especially the part indicating the early trials in learning. The shortest and most regular is the one plotted for a trial every third day. In addition, this particular curve shows that relatively few trials were required

beyond the point after the rapid descent in the curve was made. At this particular point a few more trials were necessary with one trial daily, and with one every other day still fewer were required. It seems that the longer the interval between trials, the simpler and more regular the curves become (within the limits of this experiment). As on previous curves, a small circular dot indicates the point where the first rat completed learning.

The following table, Table III, gives a summary of the trials and days found necessary to learn the latch box in the different experiments with this apparatus.

TABLE III

GIVING THE NUMBER OF TRIALS AND DAYS NECESSARY TO LEARN THE LATCH BOX

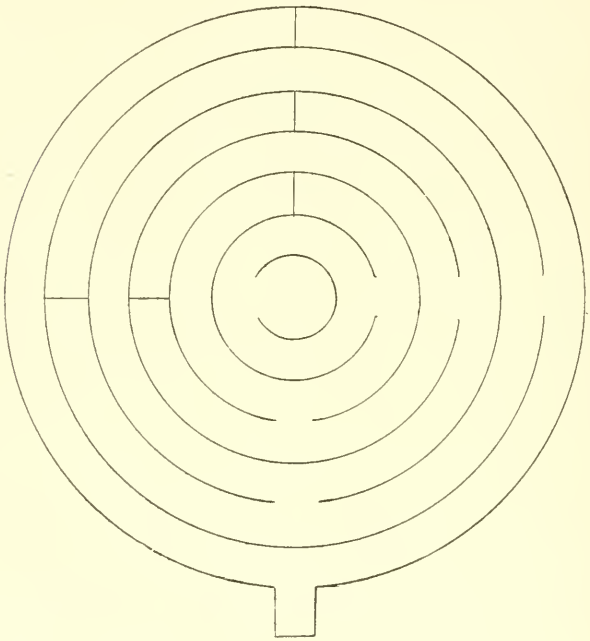
	Trial every third day	Trial every other day	Trial every day	Three trials every day	Five trials every day
No. of trials.....	11–17	13–22	14–24	24–45	35–50
No. of days.....	33–42	26–44	14–24	8–15	7–10

RESULTS WITH THE CIRCULAR MAZE

The next problem to consider is the maze, Fig. II. Though it was the last one taken up, it is convenient to treat it at this point, for it was the only other problem tried alone. This maze differs in some respects from other types in common use—the paths were circular. These were 10 cm. wide, partitioned off by vertical sheets of aluminum 12 cm. high set in grooves of a circular base 155 cm. in diameter. The center was about 20 cm. in diameter. The entrance from one path to the other and to the center were in alternate quadrants of an arc. Wire netting in two semi-circular sections covered the paths, and an additional circular piece covered the center. The distance from the entrance to the center of the maze when following a direct path was approximately 375 cm.

As in the latch-box problem, the rats were divided into groups which were given one, three and five trials. With few exceptions, the same procedure was adopted with this experiment as with the latch-box problem, but no preliminary training was undertaken. Instead of exercising the rats in a square maze during the interval before the retention tests, the rats were

FIG 11
MAZE



allowed to exercise in a 38 cm. runway that had at one end a food box. They were permitted the liberty of this runway during feeding. Some better method could have been adopted for the animals to obtain exercise if the room in which the experimentation was done had been a little larger.

The norm accepted for complete learning of the maze was an average of six seconds in two successive days, and in addition to this criterion the runs in the maze had to be relatively

free from error. Such standards, time and error-free, for learning have some interesting points worthy of notice, and their relationship to one another is rather peculiar. In the first place, since no definite agreement can be made as to what is an error, difficulties immediately arise when we come to decide when learning is complete. Gross errors, such as the animal turning in the direction of a cul de sac or retracing its steps, can readily be agreed upon as errors. Mere hesitation, however, cannot be agreed upon as an error, and in this work it was not considered such. And all the more interesting does the question of error become when its relationship to time is considered. Frequently runs in the maze were made in six seconds with several errors and this usually persisted for several days before runs in the maze were made error-free. It was not because six seconds was too high a norm, for some rats could barely make a run in six seconds and make no errors; and others, the more active, could make the run in the same time and still be charged with several errors. And this, of course, implies that *speed was first gained, then errors were eliminated.* Integration for speed seems to be first laid down, then that for the removal of errors. Probably distracting stimuli of the sort which produce unnecessary responses interfere with the simultaneous integration for speed and for runs that would be free from error.

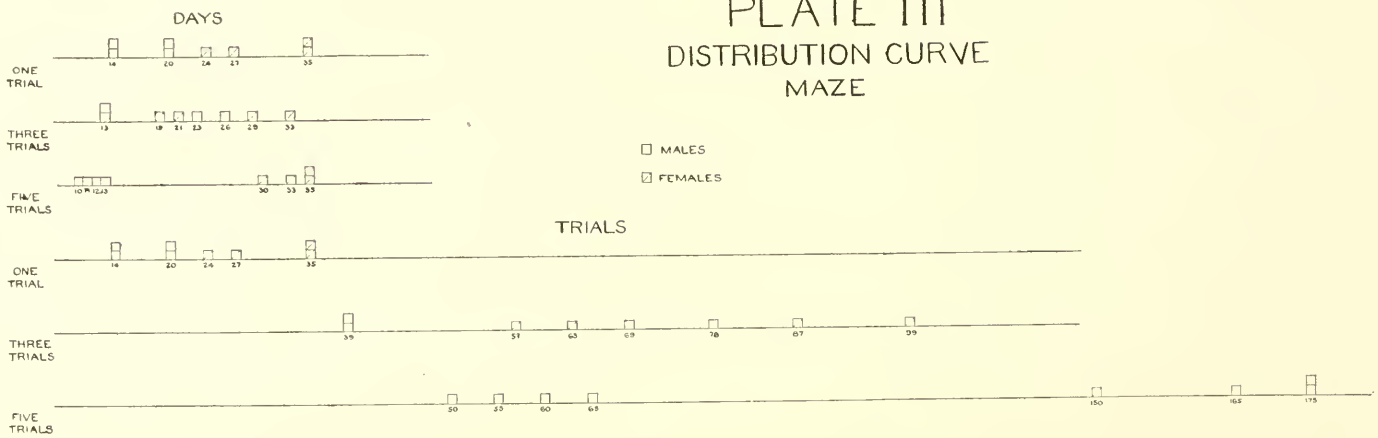
It is the dominance of hunger, acting intermittently, that evokes responses in going from position to position while both of these integrations are being laid down. Some deny that this stimulus acts in the early trials. Hicks and Carr (12) maintain in their work on the Hampton Court type of maze that the animals are at this time "governed by caution and curiosity." This scarcely proved true with this work on the circular maze. In every case, scarcely had the rats made a turn into the second or third path, and detected the odor of food, when there was immediately a noticeable eagerness to obtain it. Their activities were hardly governed by curiosity or fear. It must be admitted, however, that these activities were decidedly sporadic. The response of hunger is intermittent in character, and probably it is more intermittent during the early trials, because of the presence of numerous interfering stimuli. It is this interruption of the dominance of hunger response that made it appear that the rats in the early trials were governed by curiosity alone.

Elijah asleep under a juniper tree

PLATE III

DISTRIBUTION CURVE

MAZE



The data obtained from the maze problem are not as satisfactory as those from the latch box. Some of the rats used in this experiment were not of the same strain as those previously tested. A few of the rats, given three and five trials daily, were not perfectly normal. Their slowness of response throughout learning seemed to indicate a lack of vitality and endurance. As a result, these rats required many more trials than the more normal animals.

The presence of these animals has not, however, altered the gross results, for one trial daily appears again to be the most economical method of learning so far as trials are concerned, and five trials daily when the number of days is considered. This is clearly indicated on the distribution curve, Plate III. The first rat of the one trial group completed learning in 14 trials, that of the three and five trial groups in 39 and 50 trials respectively. On the other hand, the one, three ,and five trial groups required 14, 13, and 10 days respectively, to learn the problem. These rats may be considered normal. They were the first rats worked with when this problem was begun. The rats subjected to three trials daily requiring 69, 78, and 98 trials, respectively. and those given five trials daily requiring 150, 165, and 175 trials respectively, before reaching the norm, must be considered somewhat abnormal.

The results presented in averages are found in Table III. From these averages the curves (Curve III, p. 22), have been plotted. The individual curves for one, three, and five trial groups have the same general appearance of all curves of this kind, a rapid descent takes place after a varying number of trials, during which there is a large amount of irregularity, then a level is reached after which the final drop marking the completion of learning occurs. The initial descent in the curve for the one trial group occurs in the 11th trial, for the three and five trial groups in the 32nd and 27th trials respectively. The irregularity is most pronounced in the early trials before the descent is made. It is also particularly prominent in the curves of five trials up to the 100th trial. All this is indicative of two things: first, that where there is a slowness in descent and great irregularity there is a retardation in learning; and secondly, that this problem is a difficult one for rats not normally active.

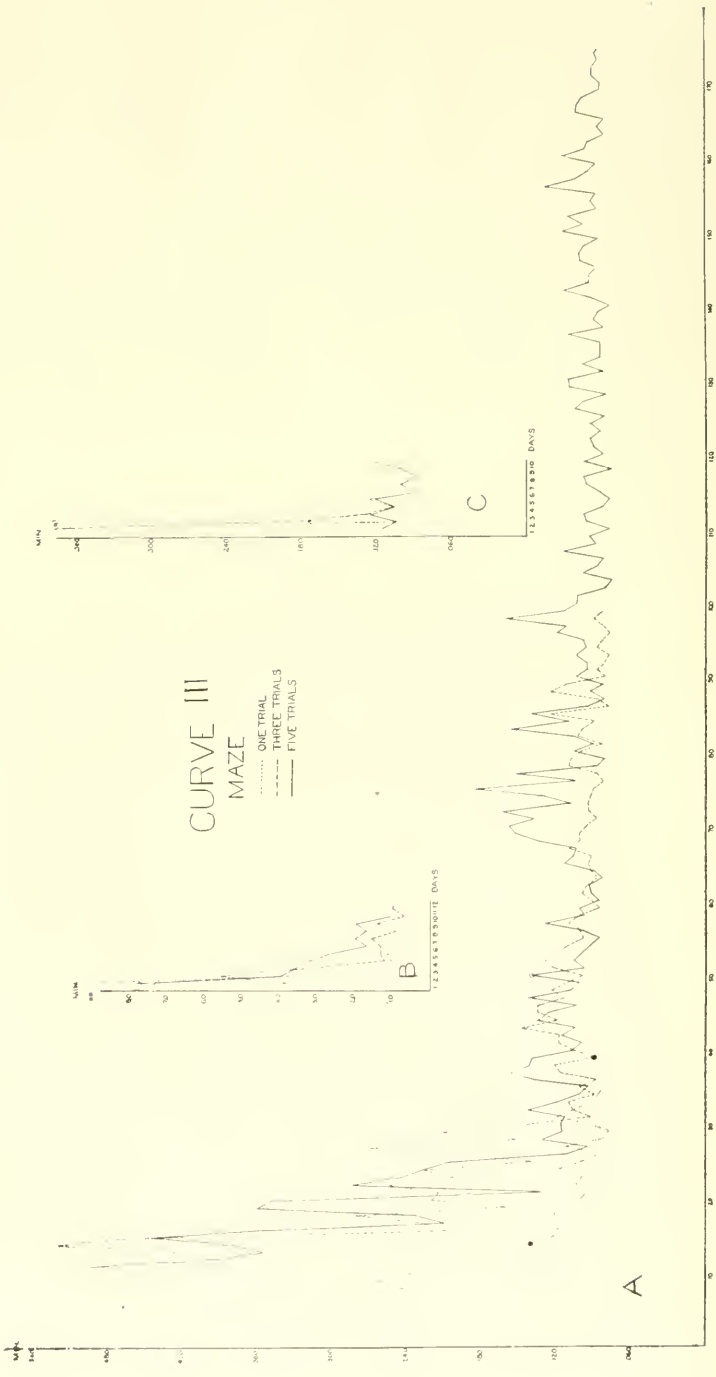
TABLE III

MAZE GIVEN ALONE

Trial No.	One trial average	Trial No.	Three trials average	Trial No.	Five trials average	Trial No.	One trial average	Trial No.	Three trials average	Trial No.	Five trials average
1	9.09	1	3.81	1	7.85			49	.112	49	.102
2	3.86	2	5.68	2	4.06			50	.099	50	.141
3	1.86	3	3.38	3	3.78			51	.121	51	.098
4	1.01	4	1.20	4	3.24			52	.128	52	.107
5	1.95	5	1.32	5	2.74			53	.120	53	.100
6	.600	6	1.32	6	1.71			54	.113	54	.094
7	.378	7	1.59	7	2.02			55	.095	55	.086
8	.243	8	.885	8	1.78			56	.102	56	.099
9	.264	9	1.15	9	1.97			57	.110	57	.129
10	.239	10	.774	10	1.17			58	.102	58	.099
11	.145	11	1.09	11	.498			59	.087	59	.102
12	.111	12	1.04	12	.387			60	.092	60	.085
13	.125	13	.417	13	.355			61	.101	61	.092
14	.140	14	.520	14	.383			62	.096	62	.094
15	.123	15	.449	15	.444			63	.087	63	.085
16	.119	16	.202	16	.360			64	.093	64	.090
17	.109	17	.233	17	.212			65	.095	65	.114
18	.121	18	.282	18	.236			66	.093	66	.107
19	.116	19	.160	19	.261			67	.110	67	.105
20	.101	20	.221	20	.349			68	.108	68	.135
21	.095	21	.215	21	.132			69	.108	69	.153
22	.104	22	.254	22	.284			70	.097	70	.158
23	.095	23	.202	23	.246			71	.087	71	.139
24	.118	24	.227	24	.229			72	.095	72	.164
25	.093	25	.139	25	.207			73	.095	73	.108
26	.085	26	.205	26	.111			74	.093	74	.130
27	.110	27	.270	27	.096			75	.091	75	.187
28	.083	28	.154	28	.132			76	.083	76	.106
29	.078	29	.212	29	.115			77	.089	77	.154
30	.095	30	.144	30	.117			78	.110	78	.089
31	.120	31	.094	31	.114			79	.108	79	.082
32	.107	32	.107	32	.143			80	.099	80	.106
33	.091	33	.110	33	.120			81	.083	81	.091
34	.087	34	.089	34	.102			82	.104	82	.105
35	.083	35	.102	35	.096			83	.100	83	.157
		36	.088	36	.138			84	.091	84	.112
		37	.117	37	.158			85	.124	85	.141
		38	.121	38	.142			86	.078	86	.095
Retention		39	.088	39	.140			87	.087	87	.082
		40	.107	40	.108			88	.124	88	.093
1	.341	41	.113	41	.099			89	.112	89	.083
2	.112	42	.113	42	.116			90	.083	90	.104
		43	.149	43	.106			91	.091	91	.091
		44	.120	44	.137			92	.083	92	.091
		45	.135	45	.127			93	.078	93	.106
		46	.135	46	.104			94	.087	94	.091
		47	.114	47	.142			95	.083	95	.099
		48	.124	48	.109			96	.078	96	.120

TABLE III—*Continued*

Trial No.	One trial average	Trial No.	Three trials average	Trial No.	Five trials average	Trial No.	One trial average	Trial No.	Three trials average	Trial No.	Five trials average
		97	.083	97	.112					144	.087
		98	.091	98	.172					145	.080
		99	.083	99	.120					146	.095
				100	.103					147	.102
				101	.103					148	.097
		Retention		102	.080					149	.083
				103	.076					150	.114
				104	.099					151	.097
		1	.597	105	.087					152	.110
		2	.192	106	.082					153	.087
		3	.130	107	.114					154	.093
		4	.109	108	.087					155	.082
		5	.126	109	.093					156	.129
		6	.115	110	.078					157	.105
				111	.091					158	.095
				112	.099					159	.089
				113	.083					160	.116
				114	.078					161	.097
				115	.086					162	.095
				116	.091					163	.080
				117	.099					164	.087
				118	.076					165	.080
				119	.099					166	.105
				120	.083					167	.103
				121	.091					168	.099
				122	.093					169	.085
				123	.085					170	.085
				124	.093					171	.091
				125	.078					172	.091
				126	.105					173	.087
				127	.099					174	.089
				128	.080					175	.085
				129	.097						
				130	.110						
				131	.087					Retention	
				132	.099						
				133	.085					1	.117
				134	.085					2	.105
				135	.085					3	.127
				136	.111					4	.108
				137	.082					5	.124
				138	.091					6	.091
				139	.085					7	.101
				140	.078					8	.089
				141	.093					9	.098
				142	.114					10	.123
				143	.095						


CURVE III
MAZE
ONE TRIAL
THREE TRIALS
FIVE TRIALS
A
B
C

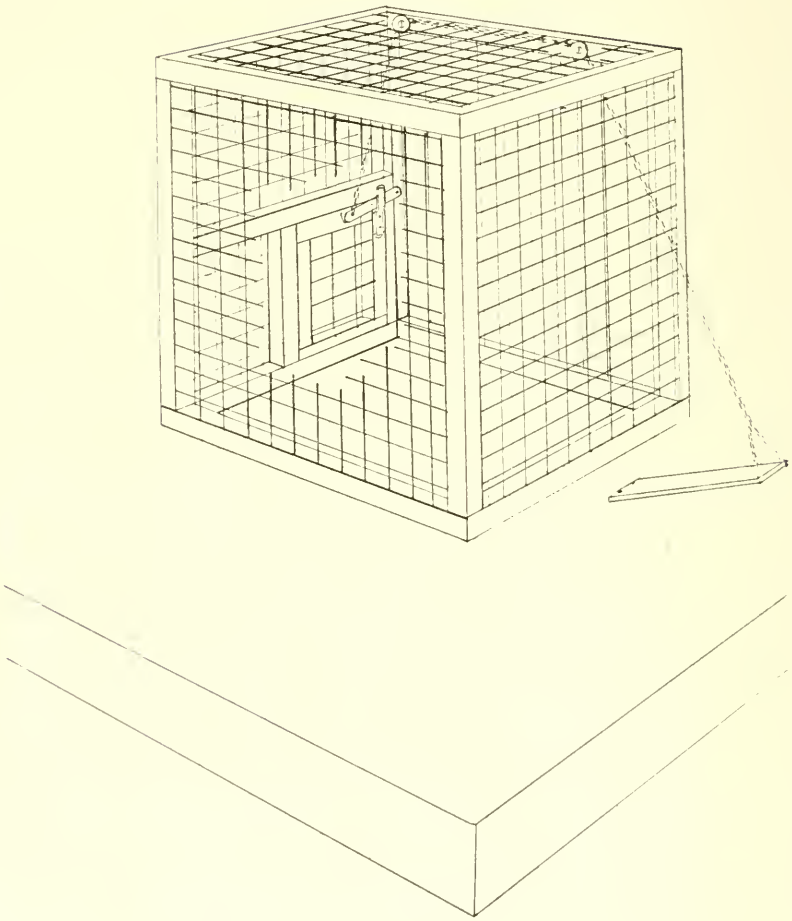
Before leaving the discussion of the maze, it will be well to compare the results and the curves of the latch box with those of the maze. In both cases learning was more economically attained (with respect to number of trials) when trials were distributed over several days, than when accumulated in one day. On the other hand, fewer days were required to learn a problem when several trials were given. The curves of the maze show some interesting differences from those of the latch box; the individual curves of the maze are longer and more irregular than those of the latter. The increased length of the curves of the maze is, beyond doubt, due in part to the fact that an additional criterion, "error free," has been added to that of time in determining the norm of the maze, and it also is due to the fact that the maze is a more difficult problem to learn, though it is said to be one where more natural movements are demanded. It is certain that the rats would have required fewer trials and the curves, as a consequence, would have been shorter, if time alone had been considered. The length of the curves of the maze would then probably approximate those of the latch box. The irregularities in the curves, however, would still remain.

II. THE EFFECT OF DIFFERENT DISTRIBUTIONS OF PRACTISE WHEN THREE PROBLEMS ARE LEARNED ABREAST

The previous section shows that when a single problem is learned, one trial daily or one trial even at longer intervals, is more advantageous than a larger number of trials given every day. The question next arises as to whether this same relation obtains under more complicated conditions of learning. In the education of children several subjects have to be learned simultaneously or abreast. It is easy to arrange similar conditions in the animal work by forcing them to learn three problems concurrently. We chose for this work, first the latch box, second the maze, both of which have already been described, and third a new problem, viz., the inclined plane box.

The inclined plane box, Fig. III, in general construction and dimensions resembles the latch box, with the exception that the latch was inside of the box and was released in a different manner. A wire chain connected with the latch passed over two pulleys attached to opposite corners of the upper frame work

FIG. III.
INCLINED PLANE BOX



inside of the box, then through the mesh of the wire netting covering the box to an inclined plane behind the box. A rat stepping on the plane would release the latch, and the door would open inward. The plane was 17 cm. long and 7.5 cm. wide and was hinged to the top of a table on which the box rested. It was about 7 cm. behind the box, and made an angle of 15 degrees with the table top when the door was closed. Like the latch box, the inclined plane box was covered with a wire hood.

We arranged three groups of animals to work upon these problems. One group was given one trial a day on each of the three problems in the order: latch box, maze, inclined plane box. The second group was given three trials per day on each problem, and the third group was given five trials per day on each problem. The rats used in this experiment were of a hardy strain, descendants of those employed in the first experiment on the latch box. Each group contained eighteen animals. In the beginning it was feared that the rats that were running five trials per day on each problem (i.e., fifteen trials in all) could not stand the strain of so many tests; but this did not prove to be the case. The rats worked well and there was no indication of fatigue or apathy at any time (see p. 48 also). Only slight rest was allowed between the problems. Time was given after each trial to allow the animal to nibble a little food. Then the next trial was given, etc. No time was lost in transferring the rats from one problem to the next.

With the three problems no methods were adopted that had not been employed with the maze and latch box when these were under experiment alone. The same norms were used, an average of one second in two successive days for the former, and six seconds, without error, for the latter. For the new problem, the inclined plane box, a norm of two seconds was adopted. The rats were required to work at these three problems until all three norms were reached. When this had been attained, they were forced to exercise daily in the 38 cm. runway until ready for retention test.

Two sets of data ought to appear in making these tests: (1) data on the effect of the different distributions and (2) an answer to the question as to whether, from the standpoint of the number of trials involved it is more efficient to learn one problem at a time or to learn several abreast.

RESULTS SHOWING EFFECT OF DISTRIBUTION OF PRACTISE

As may have been predicted, a given problem, when learned concurrently with two others, induces in the animal a different type of behavior from what it would call out if that problem were learned alone; e.g., the latch box when learned with two other problems was far more difficult than when it was learned alone. It was the first problem offered on any given day. Many more random movements appeared under these more complex conditions than when it was learned alone and many unique ways of solving the problem were noticeable. The rats which had partially learned the problem would often go to the door, smell the latch and go away, and then return later and open it with their snouts. Others would stand before the door and move their heads to and fro before making the decisive movements which would open the door. At times there appeared a degree of helplessness not evident when the latch box was offered alone. Further, with the maze, nothing very significant was noticeable with two additional problems, with the exception that hesitations were frequent at the entrance of the maze, and errors were rather frequent throughout learning. Since the inclined plane box was not taken alone, but only in connection with other problems, no criticism can be made of the behavior of the rats.

A few points in regard to the learning of the inclined plane box are of interest. The movements of the rats were more complex than those made in learning either the maze or the latch box. At first the plane was set off accidentally by the rats, but on the third or fourth day the first indication of an association of the plane and the getting of the food or the opening of the door was established. At this time, the rats went to the plane and then turned away, or in their wanderings when they came near it they avoided it. Not until the sixth to the eighth day did the first indication appear that some progress in learning was being made; the rats then went to the plane and pushed it down. It was the first precise movement made, and first appeared in those animals given one trial daily. It was not until some time later that the rats, after having learned this precise movement, immediately responded to the noise of the opening of the door and ran to get food. Apparently an additional integration

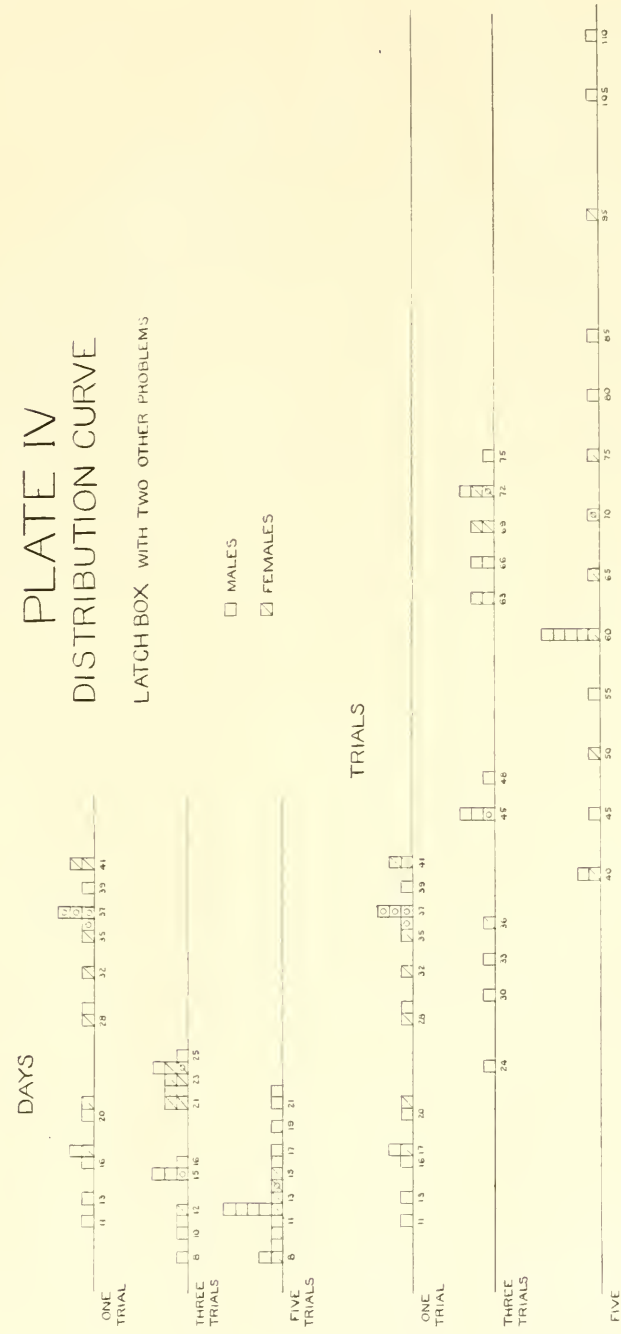


PLATE IV
DISTRIBUTION CURVE
LATCH BOX WITH TWO OTHER PROBLEMS
MALES
FEMALES
DAYS
TRIALS
ONE TRIAL
THREE TRIALS
FIVE TRIALS

of movements had to be laid down before response could be immediate. When this had taken place, learning was rapid.

The data obtained from these experiments are quite similar to those obtained when the problems were given singly. One trial daily was the most economical proceedure when either one problem or several were offered. The number of trials and the number of days required by each rat to learn the problems, are given on the distribution curves, Plates IV, V, and VI. There is one feature noticeable on these curves that was not so evident on previous distribution curves; individual differences are more strikingly shown, and from this we infer that learning is more difficult when several problems are taken concurrently than when one is learned. The number of trials and days required for the first rat to complete learning with the one, three, and five trial groups for the latch box was respectively as follows: trials 11, 24, and 40; days 11, 8, and 8; for the maze the number of trials was 27, 36, and 50, and the number of days 27, 12, and 10; for the inclined plane box the number of trials was 22, 33, and 50, and the number of days 22, 11, and 10.

The curves for the integration of movements, Curves IV, V, and VI, have been constructed from averages presented in Tables IV, V, and VI. These curves of the latch box, maze, and inclined plane box vary as do others of their kind in respect to their complexity and irregularity, and Curve VI of the inclined plane box shows these variations most clearly. It has proved to be the most difficult of the problems. The rats, in order to learn this problem, must form, as has been said, two integrations for precise movements; first, one for pushing down the plane, and, secondly, one for responding to the noise of the opening of the door in order to obtain food. The maze, seems to be less difficult than the inclined plane box, and the latch box the least difficult when making like comparisons with their curves.

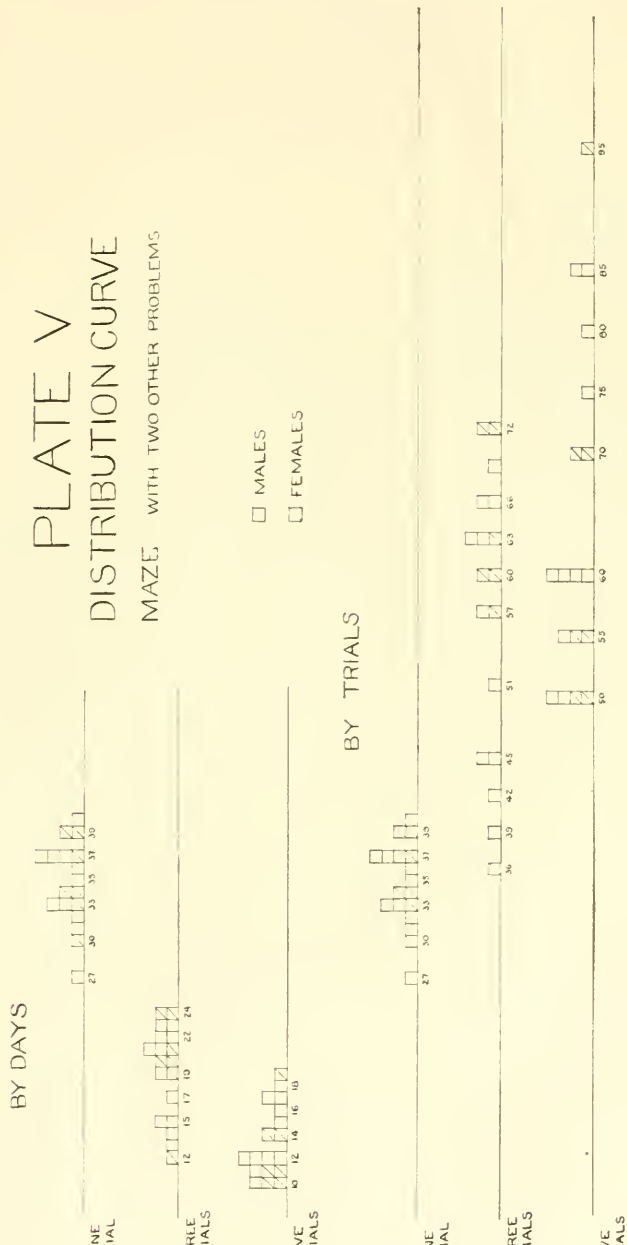


PLATE V
DISTRIBUTION CURVE
MAZE WITH TWO OTHER PROBLEMS
MALES
FEMALES
BY DAYS
ONE TRIAL
THREE TRIALS
FIVE TRIALS
BY TRIALS
ONE TRIAL
THREE TRIALS
FIVE TRIALS

PLATE VI

DISTRIBUTION CURVE

INCLINED PLANE WITH TWO OTHER PROBLEMS

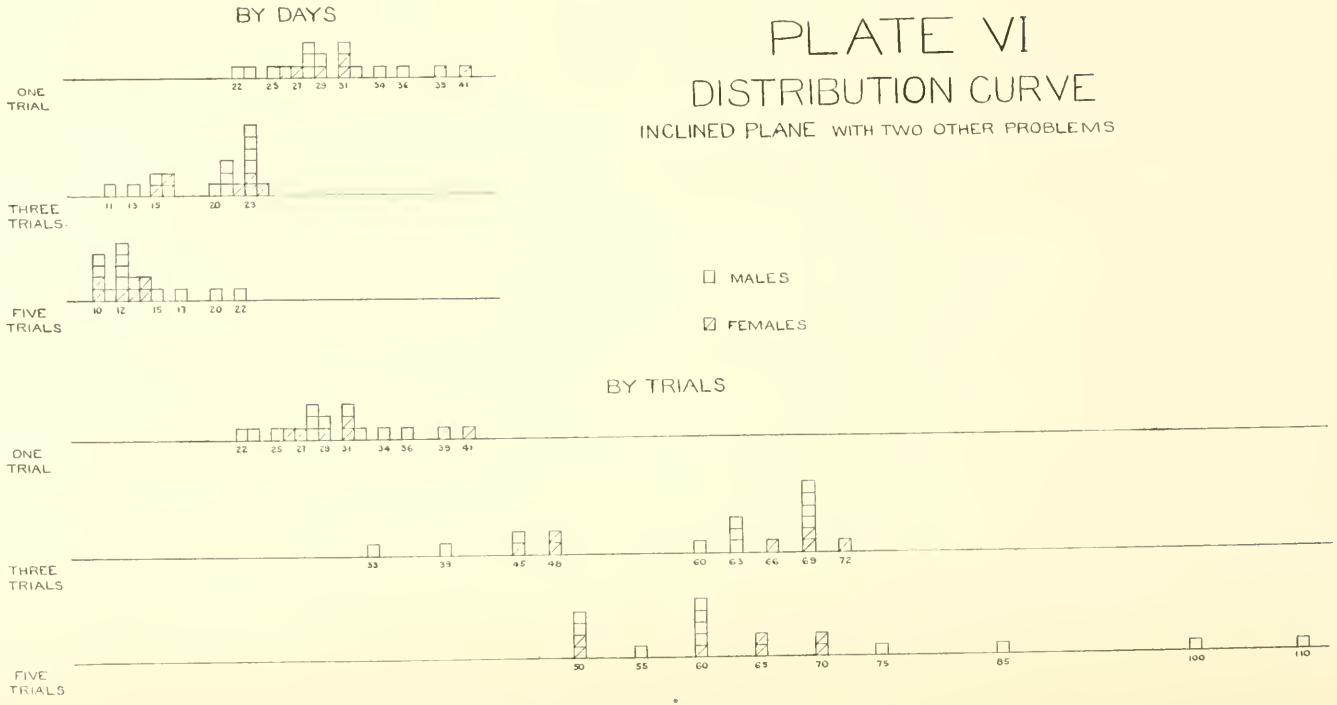


TABLE IV

LATCH BOX WITH TWO OTHER PROBLEMS

Trial No.	One trial average	Trial No.	Three trials average	Trial No.	Five trials average	Trial No.	One trial average	Trial No.	Three trials average	Trial No.	Five trials average
1	5.24	1	5.04	1	2.71			51	.018	51	.036
2	2.83	2	4.18	2	.858			52	.025	52	.024
3	1.68	3	.888	3	.498			53	.036	53	.019
4	.665	4	2.77	4	.540			54	.029	54	.022
5	1.46	5	.539	5	.505			55	.022	55	.020
6	.770	6	.415	6	.306			56	.023	56	.050
7	.265	7	.468	7	.217			57	.030	57	.024
8	.325	8	.168	8	.136			58	.017	58	.022
9	.396	9	.079	9	.108			59	.030	59	.023
10	.396	10	.186	10	.102			60	.022	60	.031
11	.138	11	.058	11	.137			61	.019	61	.019
12	.151	12	.158	12	.066			62	.017	62	.021
13	.146	13	.154	13	.060			63	.020	63	.012
14	.102	14	.057	14	.087			64	.015	64	.017
15	.091	15	.052	15	.085			65	.019	65	.018
16	.079	16	.075	16	.088			66	.019	66	.022
17	.103	17	.045	17	.056			67	.014	67	.022
18	.146	18	.041	18	.036			68	.018	68	.020
19	.143	19	.072	19	.031			69	.017	69	.020
20	.082	20	.037	20	.039			70	.014	70	.017
21	.085	21	.071	21	.081			71	.016	71	.033
22	.091	22	.075	22	.031			72	.013	72	.025
23	.083	23	.056	23	.050			73	.013	73	.020
24	.085	24	.046	24	.035			74	.013	74	.013
25	.041	25	.043	25	.040			75	.018	75	.022
26	.048	26	.056	26	.060			76	.016	76	.024
27	.068	27	.050	27	.039			77	.016	77	.047
28	.040	28	.055	28	.036			78	.016	78	.036
29	.062	29	.063	29	.044					79	.039
30	.032	30	.031	30	.036					80	.017
31	.036	31	.030	31	.095					81	.022
32	.040	32	.026	32	.058			Retention		82	.012
33	.025	33	.028	33	.044					83	.016
34	.023	34	.029	34	.031			1	.140	84	.032
35	.017	35	.029	35	.046			2	.039	85	.022
36	.019	36	.027	36	.040			3	.042	86	.027
37	.016	37	.027	37	.031			4	.037	87	.021
38	.022	38	.044	38	.028			5	.028	88	.032
39	.019	39	.037	39	.020			6	.023	89	.024
40	.016	40	.019	40	.030					90	.019
41	.013	41	.025	41	.030					91	.024
42	.013	42	.036	42	.021					92	.021
43	.016	43	.028	43	.029					93	.019
		44	.023	44	.018					94	.019
		45	.026	45	.021					95	.013
		46	.024	46	.032					96	.012
Retention		47	.023	47	.023					97	.016
		48	.024	48	.020					98	.016
1	.103	49	.035	49	.030					99	.012
2	.028	50	.019	50	.020					100	.016

TABLE IV—*Continued*

Trial No.	One trial average	Trial No.	Three trials average	Trial No.	Five trials average	Trial No.	One trial average	Trial No.	Three trials average	Trial No.	Five trials average
				101	.020					121	.016
				102	.116					122	.016
				103	.012					123	.016
				104	.016					124	.012
				105	.012					125	.016
				106	.012						
				107	.016						
				108	.024					Retention	
				109	.012						
				110	.016						
				111	.016					1	.169
				112	.012					2	.052
				113	.016					3	.026
				114	.012					4	.057
				115	.012					5	.088
				116	.012					6	.058
				117	.020					7	.020
				118	.012					8	.030
				119	.016					9	.048
				120	.016					10	.039

TABLE V

MAZE WITH TWO OTHER PROBLEMS

Trial No.	One trial average	Trial No.	Three trials average	Trial No.	Five trials average	Trial No.	One trial average	Trial No.	Three trials average	Trial No.	Five trials average
1	14.2	1	8.28	1	8.96			51	.097	51	.085
2	2.71	2	4.07	2	6.76			52	.085	52	.088
3	1.51	3	2.77	3	3.74			53	.088	53	.084
4	1.45	4	2.93	4	3.18			54	.088	54	.089
5	1.11	5	1.83	5	2.28			55	.082	55	.095
6	.980	6	1.52	6	.789			56	.088	56	.087
7	.803	7	.787	7	.620			57	.087	57	.094
8	1.120	8	.577	8	.535			58	.082	58	.097
9	.293	9	.588	9	.647			59	.093	59	.110
10	.307	10	.596	10	.606			60	.087	60	.096
11	.264	11	.369	11	.402			61	.080	61	.086
12	.266	12	.330	12	.269			62	.087	62	.089
13	.227	13	.260	13	.302			63	.084	63	.085
14	.258	14	.222	14	.301			64	.081	64	.095
15	.198	15	.286	15	.283			65	.084	65	.089
16	.282	16	.168	16	.166			66	.085	66	.086
17	.206	17	.252	17	.238			67	.082	67	.083
18	.168	18	.198	18	.229			68	.085	68	.090
19	.155	19	.266	19	.178			69	.084	69	.090
20	.149	20	.185	20	.199			70	.082	70	.092
21	.142	21	.134	21	.126			71	.085	71	.091
22	.147	22	.189	22	.124			72	.082	72	.093
23	.137	23	.139	23	.103			73	.077	73	.090
24	.110	24	.137	24	.120			74	.080	74	.091
25	.133	25	.104	25	.125			75	.083	75	.089
26	.121	26	.114	26	.109			76	.083	76	.114
27	.105	27	.114	27	.097			77	.083	77	.086
28	.097	28	.107	28	.119			78	.083	78	.092
29	.098	29	.106	29	.105					79	.084
30	.101	30	.107	30	.121					80	.093
31	.104	31	.099	31	.111					81	.099
32	.103	32	.102	32	.127			Retention		82	.081
34	.103	33	.109	33	.105					83	.079
35	.093	34	.088	34	.116			1	.327	84	.079
36	.104	35	.096	35	.116			2	.218	85	.079
37	.084	36	.094	36	.093			3	.155	86	.083
38	.091	37	.094	37	.097			4	.126	87	.080
39	.086	38	.091	38	.091			5	.093	88	.091
40	.095	39	.093	39	.096			6	.098	89	.088
41	.085	40	.088	40	.107					90	.097
42	.083	41	.085	41	.084					91	.083
43	.080	42	.084	42	.089					92	.094
		43	.089	43	.088					93	.085
		44	.091	44	.087					94	.088
Retention		45	.085	45	.090					95	.085
		46	.084	46	.085					96	.091
		47	.102	47	.087					97	.070
1	.434	48	.092	48	.084					98	.078
2	.156	49	.095	49	.088					99	.078
		50	.080	50	.090					100	.070

TABLE V—*Continued*

Trial No.	One trial average	Trial No.	Three trials average	Trial No.	Five trials average	Trial No.	One trial average	Trial No.	Three trials average	Trial No.	Five trials average
				101	.074					121	.083
				102	.078					122	.083
				103	.070					123	.083
				104	.078					124	.083
				105	.074					125	.083
				106	.085						
				107	.083						
				108	.083						
				109	.091					Retention	
				110	.083						
				111	.078					1	.493
				112	.087					2	.181
				113	.087					3	.126
				114	.087					4	.142
				115	.091					5	.148
				116	.083					6	.142
				117	.083					7	.119
				118	.083					8	.111
				119	.087					9	.120
				120	.083					10	.099

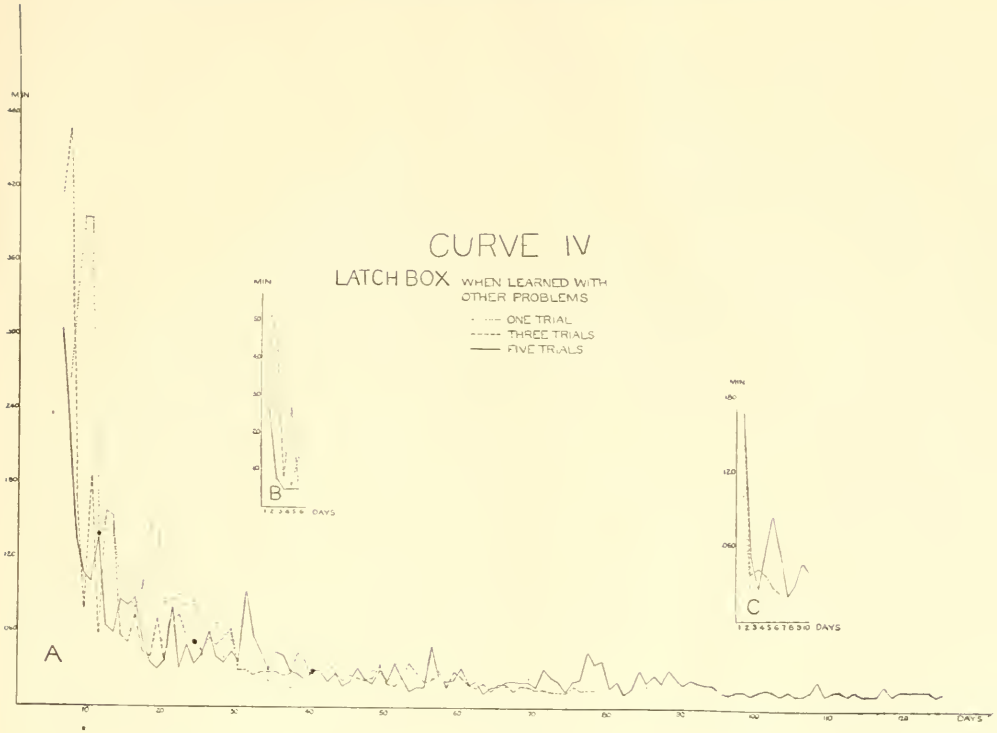
TABLE VI

INCLINED PLANE WITH TWO OTHER PROBLEMS

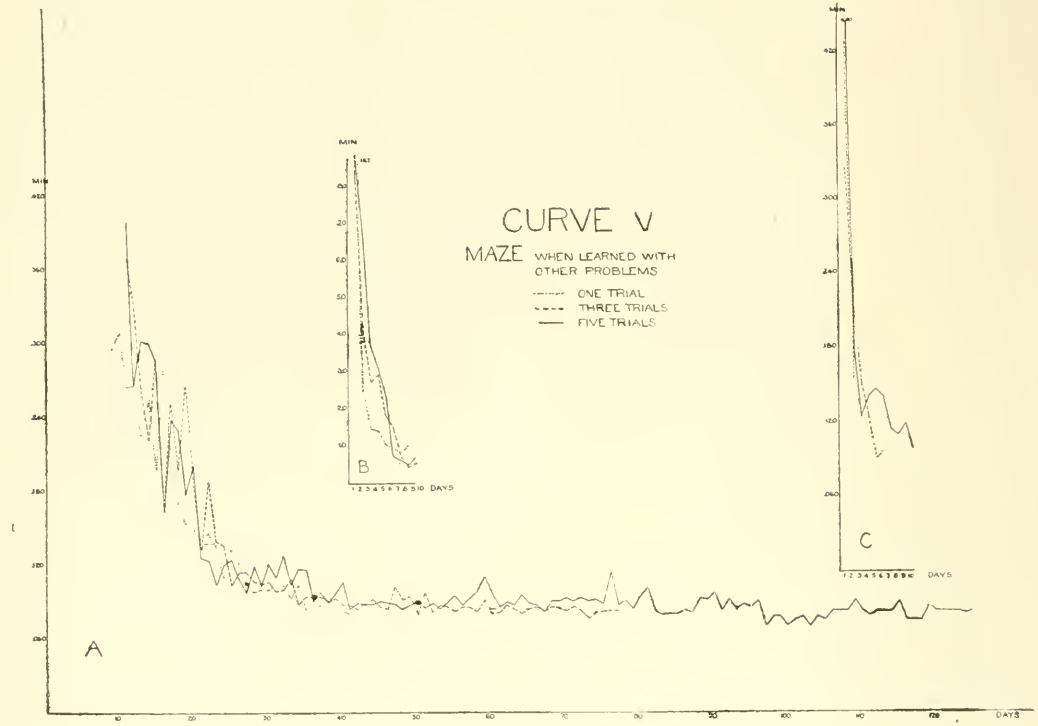
Trial No.	One trial average	Trial No.	Three trials average	Trial No.	Five trials average	Trial No.	One trial average	Trial No.	Three trials average	Trial No.	Five trials average
1	3.89	1	1.07	1	.624	2	.066	50	.084	50	.039
2	4.08	2	.560	2	.107			51	.033	51	.030
3	2.57	3	.987	3	.827			52	.109	52	.035
4	1.80	4	.815	4	.792			53	.064	53	.033
5	1.23	5	1.15	5	.937			54	.073	54	.039
6	.576	6	.922	6	.763			55	.166	55	.034
7	.841	7	.461	7	.975			56	.076	56	.038
8	1.27	8	.770	8	.819			57	.084	57	.039
9	.890	9	.771	9	.657			58	.064	58	.052
10	1.34	10	.464	10	.904			59	.056	59	.030
11	.640	11	.822	11	.312			60	.054	60	.029
12	.867	12	.317	12	.276			60	.120	61	.029
13	.484	13	.514	13	.258			62	.051	62	.023
14	.420	14	.404	14	.264			63	.039	63	.021
15	.410	15	.377	15	.431			64	.031	64	.038
16	.218	16	.371	16	.331			65	.022	65	.029
17	.180	17	.271	17	.232			66	.021	66	.040
18	.158	18	.208	18	.248			67	.034	67	.034
19	.233	19	.196	19	.173			68	.023	68	.045
20	.107	20	.139	20	.215			69	.022	69	.023
21	.081	21	.188	21	.181			70	.030	70	.053
22	.080	22	.208	22	.205			71	.029	71	.035
23	.168	23	.113	23	.187			72	.025	72	.034
24	.060	24	.140	24	.307			73	.021	73	.044
25	.077	25	.188	25	.372			74	.021	74	.032
26	.050	26	.129	26	.238			75	.021	75	.044
27	.040	27	.162	27	.101			76	.033	76	.026
28	.060	28	.180	28	.163			77	.033	77	.034
29	.060	29	.120	29	.129			78	.033	78	.039
30	.049	30	.121	30	.118					79	.022
31	.052	31	.166	31	.318					80	.039
32	.042	32	.107	32	.127					81	.032
33	.028	33	.170	33	.116			Retention		82	.029
34	.031	34	.071	34	.246					83	.027
35	.041	35	.059	35	.160			1	.178	84	.029
36	.041	36	.095	36	.093			2	.097	85	.032
37	.030	37	.112	37	.069			3	.080	86	.027
38	.034	38	.056	38	.113			4	.047	87	.044
39	.022	38	.071	39	.096			5	.038	88	.044
40	.016	40	.091	40	.102			6	.038	89	.021
41	.027	41	.059	41	.084					90	.021
42	.022	42	.055	42	.073					91	.030
43	.022	43	.133	43	.054					92	.030
		44	.115	44	.044					93	.027
		45	.080	45	.075					94	.044
		46	.122	46	.058					95	.033
Retention		47	.058	47	.052					96	.041
		48	.094	48	.049					97	.050
1	.127	49	.120	49	.045					98	.020

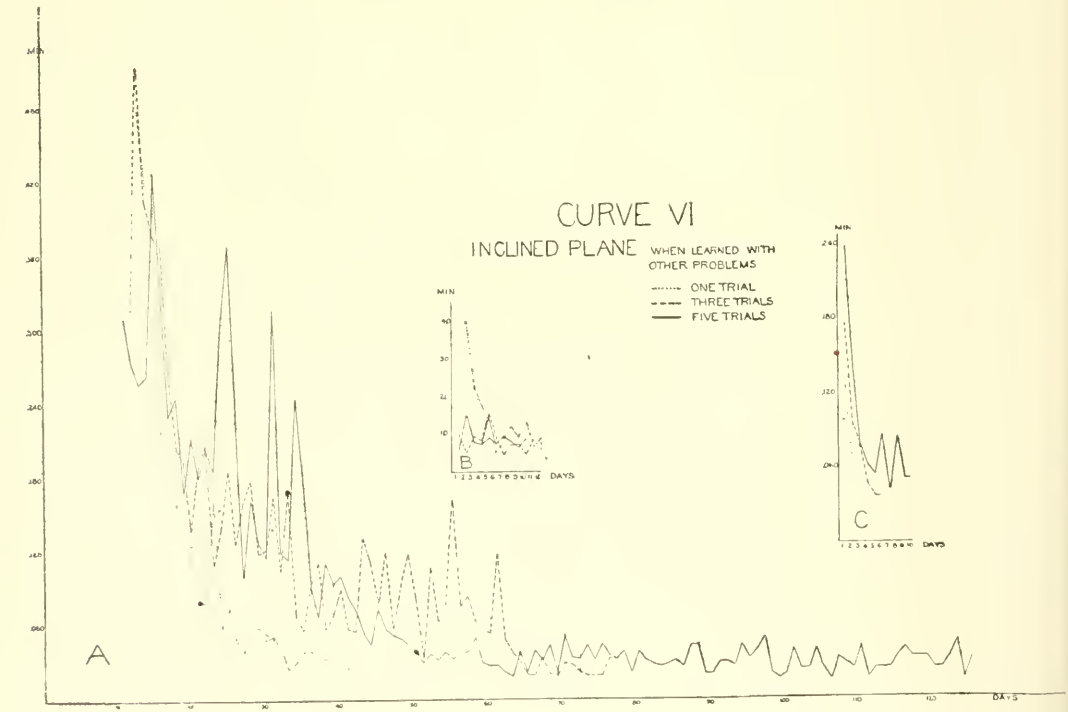
TABLE VI—*Continued*

Trial No.	One trial average	Trial No.	Three trials average	Trial No.	Five trials average	Trial No.	One trial average	Trial No.	Three trials average	Trial No.	Five trials average
				99	.016					121	.024
				100	.020					122	.033
				101	.041					123	.050
				102	.024					124	.016
				103	.024					125	.033
				104	.041						
				105	.024						
				106	.016						
				107	.033						
				108	.028						
				109	.024					Retention	
				110	.041						
				111	.016					1	.241
				112	.024					2	.140
				113	.024					3	.080
				114	.024					4	.063
				115	.033					5	.055
				116	.041					6	.089
				117	.033					7	.042
				118	.033					8	.087
				119	.033					9	.052
				120	.024					10	.053

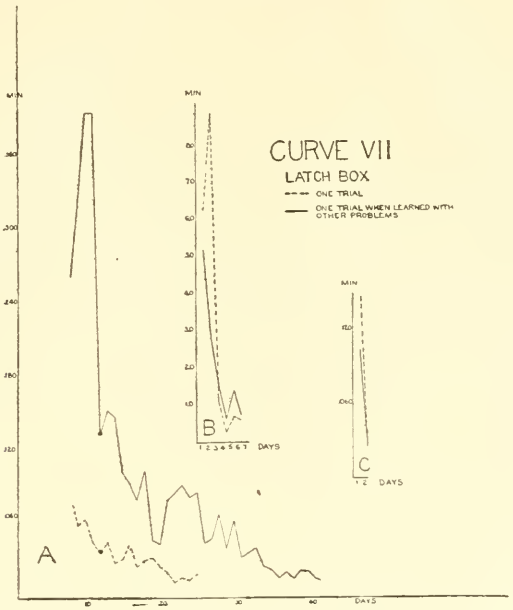


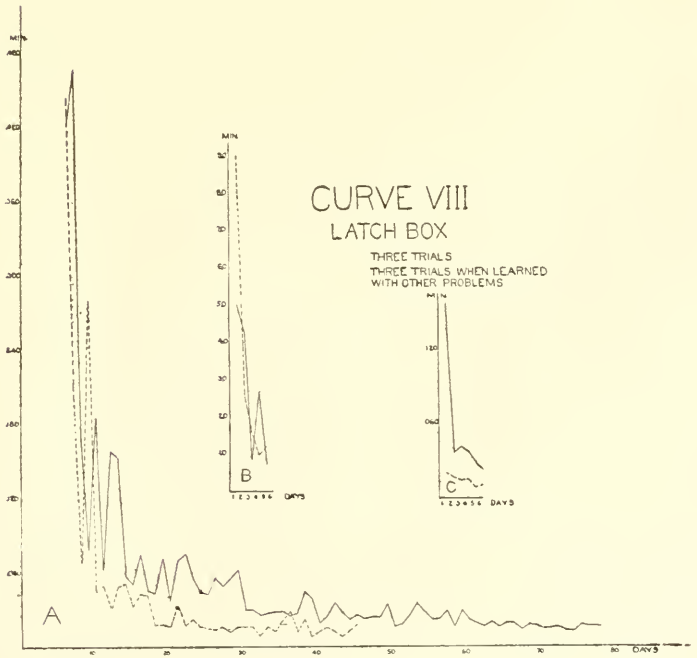
Likewise do the individual curves for the one, three, and five trial groups indicate differences in length and regularity. Again curves representing three and five trials are more irregular than those of one trial. Similar conditions it will be remembered, were recognized when one problem was offered with three and five trials. The irregularity, especially of the early trials, is more evident when several problems are learned together. In order to see this more clearly it is necessary to consult Curves VII, VIII, IX, of the latch box, and Curve X, of the maze. Curve VII shows the individual curves of the one trial groups when the latch box is given alone and when it is presented with two additional problems. In a similar way Curves VIII and IX present the three and five trial groups. Curve X gives the one trial groups of the maze. It is apparent on examining these curves that when one problem is learned the curves are shorter and simpler. This might be expected when consideration is given to the fact that apparently when several problems are

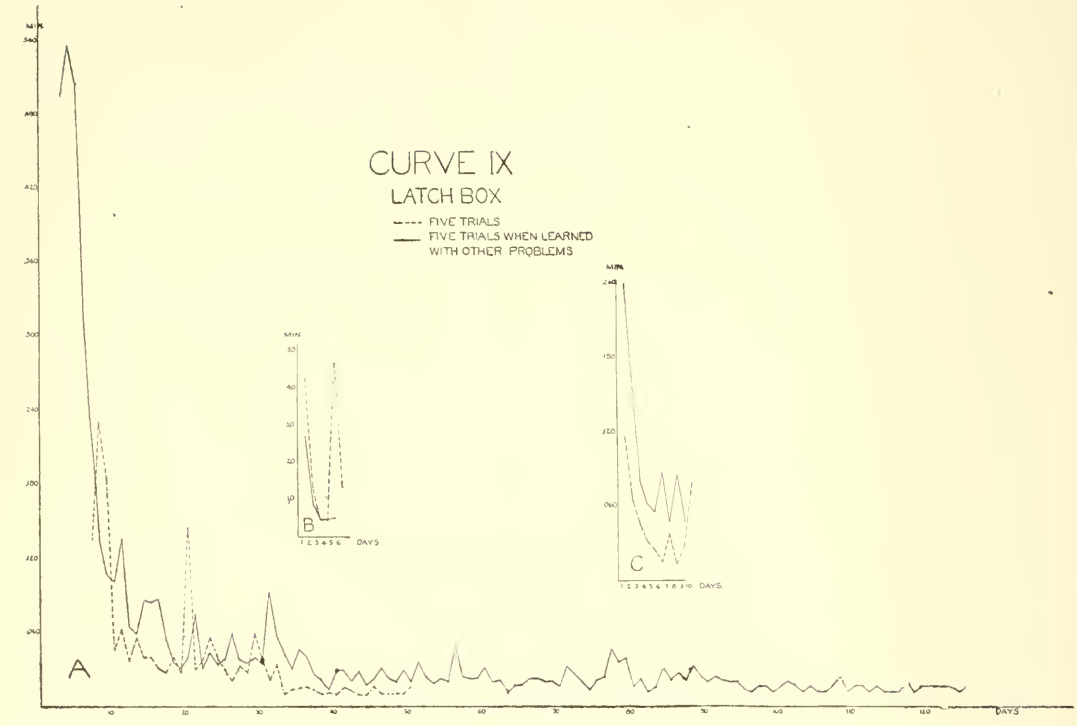

CURVE V
MAZE WHEN LEARNED WITH OTHER PROBLEMS
ONE TRIAL
THREE TRIALS
FIVE TRIALS
A
B
C
DAYS

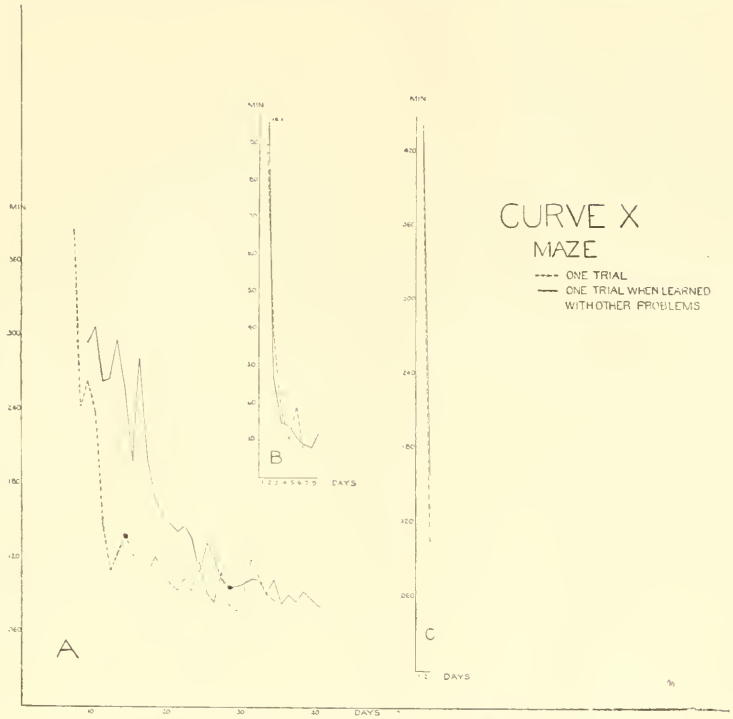

CURVE VI
INCLINED PLANE WHEN LEARNED WITH OTHER PROBLEMS
ONE TRIAL
THREE TRIALS
FIVE TRIALS
A
B
C
DAYS

given abreast the behavior of the animal becomes more complex than when the problems are learned singly. More integrations are evidently thus demanded.






CURVE IX
LATCH BOX
FIVE TRIALS
FIVE TRIALS WHEN LEARNED WITH OTHER PROBLEMS
A
B
C
DAYS



SUMMARY OF EXPERIMENTS ON LEARNING

The results obtained from all the experiments of this study point to the conclusion that a trial given every third day is the most economical as regards the number of trials. In other words, when learning is distributed over several days, and not accumulated in one day, the total time of learning is the shortest, and less effort is demanded of the organism. Furthermore, one trial given daily, though not so profitable in learning as a trial given at slightly longer intervals, remains more economical than when several trials are given concurrently . On the other hand, when several trials are given, fewer days are necessary to learn the problems, which makes this method the most economical when considering the number of days. Although there is a saving in the number of days with more trials, learning is increasingly difficult, and hence greater effort on the part of the organism is demanded. As has been pointed out, this is clearly shown by the complexity of the curves. The following table will show these facts more concretely.

TABLE VII

TABLE GIVING THE NUMBER OF TRIALS AND DAYS NECESSARY TO LEARN ALL PROBLEMS

Distribution of Trials	When One Problem is Learned Alone				When Three Problems are Learned Abreast					
	Latch-box		Maze		Latch-box		Maze		Inclined Plane	
	Trials	Days	Trials	Days	Trials	Days	Trials	Days	Trials	Days
Trial every third day	11–17	33–51								
Trial every other day	13–22	26–44								
One trial every day	14–24	14–24	14–35	14–35	11–41	11–41	27–40	27–40	22–41	22–41
Three trials a day	24–45	8–15	39–99	13–33	24–75	8–25	36–72	12–24	33–72	11–24
Five trials a day	35–50	7–10	65–175	13–35	40–120	8–22	50–95	10–19	50–120	10–22

III RETENTION

Very little has been said about the factor of retention in the rats for the reason that prior to any analysis of the results so far obtained, it seems desirable to consider the factors that determine retention. These are of two kinds: those that are dependent on the general condition of the organism and those that arise from usual extraneous disturbances. The first of these factors is connected with the problem of general metabolism. The important one, the control of diet with exercise, during the interval of the last trial and the first retention test, need only be mentioned. There is great danger at this time of overfeeding and thus defeating the results obtained by an increase in weight and the evils arising therefrom. In order to keep down body weight, daily exercise is in a measure effective and can be easily instituted, but some difficulty in regulating food is often encountered, one animal may gain fat rapidly on an amount of food that would not be at all sufficient for another. To maintain the proper bodily tone necessary to produce active organisms is of the first importance in retention tests.

Among the second class of factors, extraneous disturbances, may be noted those arising from accidental noises, bites of parasites, unusual olfactory stimulation, etc. These disturbances are of the same kind as those met with in the learning process. For some reason, however, the animals during retention tests seem to be more sensitive to such disturbing stimuli than they are during the learning process. Why this should be so seems hard to answer.

Whatever the cause of such disturbances the fact remains that it is difficult to state by observing the amount of time the first retention test requires just what proportion of the excess time must be assigned to actual loss through disuse and what to disturbing factors.

But in some cases a loss in retention was unmistakable, and was often noticed in particularly interesting points in the solution of the problems. In all three problems experimented with the rats showed evident loss in retention by wandering about aimlessly for a time, then suddenly and unexpectedly attacking the problems. Hesitations were frequent. In the latch box they were often noticed in the many positions the rats took before making an immediate movement to lift the latch. Some

rats went without hesitation to the latch, held their snouts under it, but for a short time made no decisive movement to raise it. A similar loss in retention was recognized in the solution of the inclined plane box when some of the rats went to the plane but did not push it down. In the maze there was an increase in the time of the runs and there was a reappearance of errors. With some a slowness of action alone was evident when rats were first given the problem, but, when an initial start was made, speed was regained. In all these cases a loss in retention was particularly evident. In all such cases it was apparent that there had been an actual interference with or loss of the integrations. But interference was not always present, because for some individual rats retention was perfect. The integration of movements was as perfect as that displayed in the last trial in the original training series, and the time was the same. This statement must be restricted to the latch box and the inclined plane box, the retention for the maze cannot be said to have been ever perfect, for errors were always present after a period of no practice.

As in the case of learning, only average results are given and these are presented on Tables I, II, III, IV and V for retention. The averages of the last trial, the first retention test, and the mean averages of both are offered. Upon examining these tables critically when one problem was given with one or more trials the retention test averages are higher than the averages of the last trial. The same thing is true when three problems are given concurrently, the tables here presenting great difference in this respect. There is no exception to this statement. In regard to the effect of varying the number of trials, the results on retention vary. When the latch box was used alone retention was best with three trials. On the other hand when two additional problems were given with the latch box, the retention average was best with one trial. With the maze alone the average was lowest with five trials. When the maze was used with two additional problems the three trials method was best. With the inclined plane box the retention was best when one trial was given.

Little further can be said about these results for they do not point to anything very definite. Before anything conclusive can be said, better methods for the preparation of the tests

are necessary, and probably many more animals will have to be employed.

RETENTION TABLE I

Latch Box Alone

The group showing the best average on the retention test is marked with an X. An X is also for convenience in reading placed before the figures showing the average.

No. of animals	No. of trials given	Last practice tria' Av.	Av.M.V.	First retention test Av.	Av.M.V.	Problem
17	1	.014	± .006	.082	± .074	Latch box alone
11	×3	.016	± .007	×.022	± .008	Latch box alone
11	5	.011	± .005	.117	± .144	Latch box alone
13	1 in 2 days	.014	± .004	.205	± .205	Latch box alone
9	1 in 3 days	.012	± .004	.056	± .040	Latch box alone

TABLE II

Latch Box with Two Other Problems

18	×1 on each	.018	± .004	×.103	± .091	1 trial on latch box, 1 trial on maze, 1 trial on inclined plane
18	3 on each	.013	± .003	.140	± .179	Same as above except 3 trials on each
18	5 on each	.013	± .004	.169	± .100	Same as above except 5 trials on each

TABLE III

Maze Alone

8	1	.092	± .010	.341	± .187	Maze
8	3	.091	± .010	.596	± .460	Maze
8	×5	.087	± .008	×.271	± .121	Maze

TABLE IV

GIVING SUMMARY OF RETENTION TESTS

MAZE WITH TWO OTHER PROBLEMS

No. of animals	No. of trials given	Last practice trial Av.	Av.M.V.	First retention test Av.	Av.M.V.	Problem
18	1	.079	± .006	.509	± .245	Maze with 2 other problems
18	3	×.081	± .005	×.327	± .180	Maze with 2 other problems
18	5	.086	± .006	.493	± .446	Maze with 2 other problems

TABLE V

INCLINED PLANE WITH TWO OTHER PROBLEMS

18	×1	.029	± .095	×.127	± .091	Inclined plane with 2 other problems
18	3	.022	± .007	.158	± .167	Inclined plane with 2 other problems
18	5	.028	± .016	.241	± .208	Inclined plane with 2 other problems

DISCUSSION OF RESULTS

That one trial daily or at slightly longer intervals requires fewer trials to complete learning or an integration is of some physiological interest. One might argue that, after the first trial, if the rat had had a little food, a second trial, immediately following would heighten the tendency to perform the same movements, and this tendency would be effective in all succeeding trials up to a point when the animal became slightly fatigued. Such a hypothesis would be similar to physiological summation of stimuli in the local functioning of reflexes. But results prove that anything approaching such an explanation is untenable; one daily trial is economically more effective than several trials given concurrently on the same day.

There remain two possible explanations why one trial a day or at greater intervals is the most economical. First, it may be supposed in opposition to the above that several trials a day might produce fatigue; secondly, that they might produce too pronounced metabolic changes. In regard to fatigue no objective manifestations of its presence were noticeable; for there were no differences in the responses evoked at each trial, when several were given, that would indicate that the last trial was performed with more difficulty than the first. The rats were active throughout all trials given for one day. Moreover, the last trial required often less time for the solution of the problem than the first. This can be seen in Tables IX and X, where daily records for three days are given of the three and five trial groups with one problem and with three abreast. The shortest trial on any day is often shown to be the last trial given on that day. Even when three problems were given abreast with five trials for each problem—a total of fifteen trials a day—the last few trials of the fifteen were oftentimes performed with a similar facility. The short break in this case in passing from one of the problems to the other can scarcely be counted as periods of rest.

The second explanation that several trials cause too active metabolic changes is as near the truth as can be reached at the present time. It is possible that with one trial a day or at greater intervals just sufficient transformation of energy occurs to make every trial physiologically effective, whereas, when several trials are given concurrently, there is a too rapid and extensive transformation of energy. The necessary anabolic changes after each trial are not permitted to take place before the other trials follow, and, as a result, katabolic changes increase to such an extent that the effectiveness of each trial is impossible to establish. Probably the first of several trials is physiologically the most effective, the others being proportionally less so. To produce a change in the animal so that learning will be most economical, one trial daily or at greater intervals is the best.

TABLE VIII

LATCH-BOX

No. of Trials	Rat No.*	2nd Day	3d Day	4th Day
Three trials daily	I	083-.016-.024	.100-.041-.050	.016-.016-.024
	III	.024-.066-.024	.012-.016-.012	.133-.016-.012
Five trials daily	IV	.033-.033-.024-.033-.033	.033-.024-.016-.016-.100	.016-.008-.008-.008-.008
	IX	.016-.024-.016-.008—.016	.016—.024-.008-.016-.016	.024-.024-.016-.016-.016

LATCH-BOX WITH TWO OTHER PROBLEMS

No. of trials	Rat No.	8th Day	9th Day	10th Day
Three trials daily	IX	.050-.116-.033	.066-.016-.016	.024-.016-.016
	XVIII	.083-.033-.016	.016-.041-.016	.050-.041-.033
No. of Trials	**Rat No.**	**3rd Day**	**4th Day**	**5th Day**
Five trials	V	.066-.066-.050-.041-.033	.166-.050-.050-.033-.033	.083-.033-.050-.066-.083
	XII	.033-.016-.016-.016-.016	.016-.033-.008-.008-.008	.050-.033-.008-.008-.008

* The records are taken at random from among a very large number.

SOME GENERAL REMARKS ON THE INTEGRATION OF MOVEMENTS

Rats are endowed with the physiological predisposition to display, upon the application of a stimulus, movements both native and acquired. Many of these, which the rats make when first confronted with a new situation or problem, appear, because of their sharpness, to be more or less impulsive and primal. They are all integrated movements. Some of them are useful and some of them are possibly of indifferent value. While they do not follow one another in any predictable manner, nevertheless there is some interconnection among the movements. The close study of Sherrington (13) on the reflexes from a spinal animal has yielded information on the linkage of the separate movements involved in very complex co-ordination. He also makes clear some of the conditions involved in both native and acquired integrations. It is probably safe to say that both types (native and acquired) of integrations have the same constructive mechanism or structural basis.

It is well known that for the accurate learning of any problem such as we have been considering a new or acquired integration must be established. In this process of acquisition the animal displays numerous sporadic movements. These are scarcely produced *de novo* by the situation. They must be native reflexes, or modifications at least, of previously existing integrations. From this mass of movement there slowly emerges the new and desired integration. In a sense, then, an acquired integration is a growth. It is usually said, further, that in this growth, or in learning, there is a process of stamping in of the useful movements, and stamping out of the useless movements. It has often been maintained that the movements giving pleasure are the ones maintained and those giving pain are eliminated. But this addition to the trial and error theory often leads to a form of anthropomorphism. While I am unable to advance any serviceable explanation of the stamping in and stamping out process, I wish to mention some objective changes in the types of movement displayed during the learning process.

The animal passes from an excitable state, during the early trials, to a less excitable one as learning is completed. The excitable state is very evident in the early trials, and is the "emotional" one so often mentioned. Many profound bodily

changes occur during this excitable state. It has been shown by Cannon (14) that during any emotional excitement there is a secretion of adrenalin. It seems that this substance in the blood causes the activities of the alimentary canal to cease; there is a change in the circulation from the great vessels of the abdomen to the lungs, limbs, and central nervous system, and the heart action increases in vigor. There is in every way a bettering of the circulation of the blood, and the animal can stand the strain resulting from emotional excitement during the early trials in learning. Moreover the increase in circulation in some way obviates fatigue in the muscles. Thus there are metabolic changes going on in the organism under stress of learning which tend not only to sustain it, but also to foster movements.

There must necessarily be some indication of these changes on the learning curves. The irregularities of the curves during the early trials represent the entire process of learning during the excitable period. It is most prominent when several trials are given daily, and when problems are given concurrently. After the early trials, when the movements have become partially integrated, that is, just after the rapid descents in the curves are made, the excitable state, to a great extent, disappears. The curves become less complex and hunger thereafter mainly dominates the action of the body as a whole. Learning is smoother, and this clearly means that there is no more clashing among opposing activities of the different parts of the body. When antagonistic reactions, the kind that produce errors, cease, and the attending excitations subside, learning can be said to be complete.

CONCLUSIONS

1. Considering first the latch box and the maze when they were learned alone, we find that one trial a day is a more efficient procedure than either three trials per day or five trials per day.

2. Tests upon the latch box show that a more infrequent practice distribution is even more efficient—one trial given every third day giving better results than one trial every day or one trial every other day.

3. Again, when three problems are learned abreast two things appear clearly: First, infrequent practice again appears to be

advantageous: those animals working by the one trial method on each of the three problems mastered them with fewer trials than the animals working either by the three trial or the five trial method. Second, when three problems are learned abreast a much larger number of trials is required to learn each of the problems than would have been required if the animal had been allowed to learn (by the same distribution of practice) only one problem at a time.

4. From these statements, it is conclusively shown that when trials are distributed over several days and not accumulated all in one day learning is then most economical, and physiologically there is less transformation of energy.

5. On the other hand when trials are distributed over several days many more days are necessary to establish a habit than when several trials are given on one day. In order that a problem or problems be learned in a few days, several trials daily, five or more, are required.

BIBLIOGRAPHY

(1) EBBINGHAUS, H. Ueber das Gedächtniss. 1885.
(2) JOST, A. Die Assoäiationsfestigkeit in ihrer Abhängigkeit von der Verteilung der Wiederholungen, *Zeit. f. Psy.*, vol. 14, pp. 436-472.
(3) LEUBA, J. H. and HYDE, W. An Experiment in Learning to Make Hand Movements, *Psy. Rev.*, vol. 12, pp. 351-369.
(4) MUNN, A. F. The Curve of Learning, *Archiv. of Psy.*, No. 12.
(5) STARCH, D. Periods of Work in Learning, *Jour. of Ed. Psy.*, vol. III, pp. 209-213.
(6) PYLE, W. H. Economical Learning, *Psy. Bull.*, vol. X, p. 73.
(7) ————. Concentrated *vs.* Distributed Practice, *Jour. of Ed. Psy.*, vol. V, p. 247.
(8) KIRBY, J. T. Practice in the Case of School Children, *Col. Univ. Contrib. to Ed.*, No. 58.
(9) WHITLEY, M. T. An Empirical Study of Certain Tests for Individual Differences, *Archiv. of Psy.*, No. 19.
(10) WATSON, J. B. Behavior, An Introduction to Comparative Psychology. 1914.
(11) YERKES, R. M. The Dancing Mouse. 1907.
(12) HICKS, V. C. and CARR, H. A. Human Reactions in a Maze, *Jour. of Animal Behavior*, vol. 2, p. 98.
(13) SHERRINGTON, C. S. Integrative Action of the Nervous System. 1907.
(14) CANNON, W. B. Recent Studies of Bodily Effects of Fear, Rage, and Pain, *Jour. of Philos.*, vol. XI, p. 162.

Directory of American Psychological Periodicals

American Journal of Psychology—Worcester, Mass.: Florence Chandler. Subscription $5. 600 pages annually. Edited by G. Stanley Hall. Quarterly. General and experimental psychology. Founded 1888.

Pedagogical Seminary—Worcester, Mass.: Florence Chandler. Subscription $5. 575 pages annually. Edited by G. Stanley Hall. Quarterly. Pedagogy and educational psychology. Founded 1891.

Psychological Review—Princeton, N. J.: Psychological Review Company. Subscription (with Psychological Bulletin) $5. 480 pages annually. Bi-monthly. General. Founded 1894. Edited by John B. Watson.

Psychological Bulletin—Princeton, N. J.: Psychological Review Company. Subscription $2.75. 480 pages annually. Psychological literature. Monthly. Founded 1904. Edited by Arthur H. Pierce.

Psychological Monographs—Princeton, N. J.: Psychological Review Company. Subscription $4. 500 pp. per vol. Founded 1895. Edited by James R. Angell. Published without fixed dates, each issue one or more researches.

Psychological Index—Princeton, N. J.: Psychological Review Company. Subscription $1. 200 pages. Founded 1895. Edited by Howard C. Warren. An annual bibliography of psychological literature.

Journal of Philosophy, Psychology and Scientific Methods—New York: Science Press. Bi-weekly. 728 pages per volume. Founded 1904. Subscription $3. Edited by F. J. E. Woodbridge and Wendell T. Bush.

Archives of Psychology—Sub-Station 84, New York: Archives of Psychology. Subscription $5. 600 pp. annually. Founded 1906. Edited by R. S. Woodworth. Published without fixed dates, each number a single experimental study.

Journal of Abnormal Psychology—Boston; Richard G. Badger. Subscription $4. 480 pages annually. Ed. by Morton Prince. Bi-monthly. Founded 1906. Entire field of abnormal psychology.

Psychological Clinic—Philadelphia: Psychological Clinic Press. Subscription $1.50. 280 pages annually. Edited by Lightner Witmer. Monthly (9 numbers). Orthogenics, psychology, hygiene. Founded 1907.

Training School Bulletin—Vineland, N. J.: The Training School. Subscription $1. 160 pages annually. Monthly (10 numbers). Founded 1908. Edited by H. H. Goddard. Abnormal child psychology.

Journal of Religious Psychology—Worcester, Mass.: Louis N. Wilson. Subscription $3. 480 pages annually. Published without fixed dates, 4 numbers constitute a volume. Founded 1904. Edited by G. Stanley Hall.

Journal of Race Development—Worcester, Mass.: Louis N. Wilson. Subscription $2. 460 pages annually. Founded 1910. Quarterly. Edited by George H. Blakeslee and G. Stanley Hall.

Journal of Educational Psychology—Baltimore: Warwick & York. Subscription $2.50. 600 pages annually. Founded 1910. Monthly (10 numbers). Managing Editor, J. Carleton Bell. (Educational Psychology Monographs. Edited by Guy M. Whipple. Published separately at varying prices. Same publishers.)

Journal of Animal Behavior—Cambridge, Mass.: Emerson Hall. Subscription $3. 450 pages annually. Founded 1911. Bi-monthly. Robert M. Yerkes, Managing Editor.

The Behavior Monographs—Cambridge, Mass.: Emerson Hall. Subscription $3. 450 pages per volume. Edited by John B. Watson. Published without fixed dates, each number a single research.

Psychoanalytic Review—N. Y. City. 64 W. 56th St. Subscription $5. 500 pages annually. Quarterly. Founded 1913. Edited by W. A. White and S. E. Jelliffe.

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The Johns Hopkins University

The Effect of Age on Habit Formation in the Albino Rat

HELEN B. HUBBERT

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CONTENTS

	Page
Acknowledgments	v
Introduction	1
Historical	1
Apparatus and Procedure	4
Experimental Results:	
Twenty-five day rats	15
Sixty-five day rats	23
Two hundred day rats	29
Three hundred day rats	33
Five hundred day rats	38
Comparison of Results for Different Ages	42
Incidental Tests:	
Effect of Sex on Rapidity of Learning	46
Day and Night Work	47
Continuation of Work after Problem has been Learned	48
Blood Relationship and Learning	51
Retention	52
Resume of Conclusions	54

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THE EFFECT OF AGE ON HABIT FORMATION IN THE ALBINO RAT

INTRODUCTION

The present investigation is concerned with the problem of the relation of the age of an animal to its learning capacity. Experiments were begun in the Psychological Laboratory of The Johns Hopkins University during the winter of 1912, and continued until the spring of 1915.

HISTORICAL

So far as the writer has been able to ascertain, practically no prolonged experimental work has been undertaken hitherto on the relation of age to learning ability, although the importance of the problem has been generally conceded.

In the field of human psychology, Munn[1] carried out a series of "substitution tests" on children in the grades, on normal school pupils, and on two elderly persons, to determine the relative rapidity of gain in ability to make the required substitutions. Her records were taken in the terms of time, and showed that although the children gained much more rapidly than the adults, their actual rate of speed at the beginning was lower, and that they did not reach the same level of efficiency within the limits of the experiment. Only two elderly subjects were used, hence too much reliability cannot be attributed to the results from the last group, but apparently, while their initial rate is intermediate between that of the children and the normal school pupils, they fail to reach the final rate attained by either of them. Munn gives neither the average nor the rate of gain for this last group, but the former was easily obtained, and appears in the table below.

Adults	— first test 42 seconds	— last test 14 seconds
Children	— first test 184 seconds	— last test 32 seconds
Old persons	— first test 72 seconds	— last test 39 seconds

[1] Munn. Curve of Learning. *Archives of Psychol.*, no. 12 p. 37.

Gain in first 5 *tests*:
Adults 16 seconds Children 34 seconds

Gain in second 5 *tests*:
Adults 5 seconds Children 14 seconds

It would appear from these results, that, if the rate of improvement is the question considered, children learn about twice as fast as adults.

Turning to the field of animal behavior we find a somewhat larger amount of experimental work on the matter under discussion, although practically all of it occurs as a side issue to some other problem. Slonaker[2] undertook a study of the normal activity of the white rat at different ages, hoping to " ascertain how the age of greatest activity compared with that at which the rats were most capable of education." His conclusions which relate particularly to the subject of this discussion are as follows:

1. " White rats of different ages show a marked difference in their activity.

2. " The very young rat and the very old rat are each noticeably inactive.

3. " These experiments indicate that the age of greatest activity ranges between 87 and 120 days.

4. " From these preliminary experiments no correlation can be made between the age at which they are most active and the age at which they learn most rapidly."

In a later paper[3] he places the age of greatest activity for the males at three hundred days, and for the females at three hundred and seventy-five days.[4] The daily activity increases with the advance in age until a certain age is reached, after which there is a gradual reduction till death occurs.[5] " The female is much more active than the male."[6] There is seen to be a

[2] Slonaker, J. R. The Normal Activity of the White Rat at Different Ages. *Journ. Comp. Neur. and Psych.*, **17** ('07), 342-59.
[3] Slonaker, J. R. The Normal Activity of the Albino Rat from Birth to Natural Death, etc. *Journ. Animal Behav.*, **II** ('12), 20-42.
[4] *Op. cit.*, p. 30.
[5] *Op. cit.*, p. 26.
[6] *Op. cit.*, p. 42.

discrepancy in the results of the two papers which Slonaker does not attempt to explain. In the latter paper as in the earlier one no attempt is made to correlate amount of activity with capacity to learn.

Yerkes[7] raised the question of the relation of age to habit formation in the dancing mouse. He worked first on the acquisition of the white-black discrimination habit, and later on the learning of simple labyrinth pathways. The indices of modifiability as given by the number of training tests required to complete the habit for dancers of one and four months respectively show that the males learned the white-black discrimination habit more quickly at one month (30 days) than at four months (120 days) while the reverse was true of the females.[8] The female was superior to the male, however, in the formation of the labyrinth habit.[9] In later work[10] he finds that male dancers ten months old learn the labyrinth more rapidly than those one to two months old, while there is practically no difference in rapidity of learning of one to two month and ten month females. The old dancers are somewhat superior to the young in their ability to learn the labyrinth paths.[11] With regard to the sensory habit he says:

"1. The dancer at one month of age acquires a particular white-black visual discrimination habit more rapidly than do older individuals. From the first until the seventh month there is a steady and marked decrease in rapidity of habit formation; from the seventh to the tenth month the direction of the change is reversed. These statements hold for both sexes.

"2. Young males acquire the habit more quickly than young females, but between the ages of four and ten months the females acquire the habit the more quickly."[12]

Haecker,[13] in work on the Mexican axolytl, found that the habit of distinguishing between wood and meat when offered to the animals in forceps, was learned with far greater difficulty

[7] Yerkes, R. M. The Dancing Mouse. The Macmillan Co., 1907.
[8] *Op. cit.*, p. 274.
[9] *Op. cit.*, p. 273.
[10] Yerkes, R. M. Modificability of Behavior in its Relation to the Age and Sex of the Dancing Mouse. *Journ. Comp. Neurol. and Psychol.*, **19** ('09), 237-271.
[11] *Op. cit.*, pp. 266-267.
[12] *Op. cit.*, p. 269.
[13] Haecker. *Arch. f. d. ges. Psych.*, **25**, 1-35.

by the young (nine month) individuals than by the old ones, whose age is not given.

Watson[14] in his Animal Education, discusses work both on habits involving simple motor ability and on those requiring skill in manipulation. He concludes that "a young rat will solve for the first time more quickly than a mature rat any problem conditioned on mere random activity, but that a problem involving associative activity and manipulation is more easily solved by the older animals." He found that with the simple saw-dust box the average time of entrance for the old rats was 85.50 minutes, while that for the young ones was 6.87 minutes and says further, "there is a gradation in the number of useless movements made by rats at different ages. At thirty-five days of age, when physical activity appears to have reached its highest stage, the percentage of useless movements is largest. As the rats grow older, this superabundant activity disappears, and in its place comes direction of activity."

To summarize the main points in this brief historical survey, we may note:

First:—That there is disagreement as to the age of greatest activity, Slonaker putting it first between eighty-seven and one hundred twenty days, and later at ten months for the males and twelve and a half months for the females, while Watson believes it to be at about thirty-five days;

Second:—That Yerkes finds the labyrinth habit more easily learned by the old dancers than by the young, while if Watson's interpretation is correct the reverse should be true;

Third:—Yerkes concludes that the female is superior to the male in learning the labyrinth.

APPARATUS AND PROCEDURE

Albino rats were chosen as subjects in this investigation, for several reasons: Slonaker, Watson and Yerkes worked with rodents, and we desired to compare our results with theirs. Nearly two hundred animals were required for actual experimental work and many more than that had to be kept on hand to provide for replacing any which might become unfit for work, and to allow for the usual losses through death and sickness. It has been found that white rats are easier to breed, handle,

[14] Watson, J. B. Animal Education. University of Chicago Press, 1903.

and care for in large numbers than any other small mammal. For reasons which will appear later, we adopted the circular maze as our problem since it is generally conceded that the rat is pre-eminent among animals in his ability to thread a labyrinth, while his satisfactoriness as a subject for experimental work is attested by the number of experimenters who have employed him in various capacities.

The rats were bred in our own laboratory as needed, inbreeding being carefully avoided and all possible care being taken to maintain uniformity of breeding conditions. All of the rats were weaned at from eighteen to twenty-three days,[15] and the sexes were separated at thirty-five to forty days and kept separate thereafter. The living cages were protected from mice and gray rats by screened compartments constructed of pine and one fourth inch wire mesh. Every two weeks the cages were thoroughly cleaned, the shelves washed with a disinfecting solution, and the rats dipped in a one per cent solution of "Kreso" to prevent the rise and spread of vermin. The animals were carefully watched and treated immediately upon the appearance of parasites, so that they were kept continually in a healthy condition. The diet consisted of milk-soaked bread given every day, and a mixture of cracked corn and sun-flower seed every other day. They seemed to thrive on this somewhat restricted diet, so that no additions were made to it although both Basset and Ulrich used carrots and fruit occasionally.

The rats were handled freely from birth, and consequently were perfectly tame and evinced no fear of the experimenter. Special care was taken to tame any rat seeming a little wild, before beginning work with him; since it was believed that fear and timidity might cause irregularities in behavior, a belief which was substantiated during the course of the experiment.

It was desired in this work to obtain not only a record of *time* but also a *distance* record of the learning process, since it was felt that this might throw considerably more light on the factors involved in learning than had yet been obtained. The maze problem seemed to offer greater possibilities in this line than either sensory problems requiring a long and tedious course of preliminary training, or problems of manipulation permitting

[15] No bad effects were noticed from this early weaning and the rats were found to be extremely active as early as the sixteenth day. See Slonaker, op. cit., p. 350.

of movements in two dimensions which would be practically impossible to trace. We therefore selected as our problem the learning of the circular maze.

Heretofore, the only data possible on such a problem have been in terms of time and errors, the time being the only reliable record since it is practically impossible to evaluate and standardize errors.[16] With regard to this Miss Hicks [17] says: "The prevalent practice of omitting all total and partial returns from the error record, and of making no attempt to evaluate varying degrees of error gives a curve which is not only worthless but false." She says further: "The total distance criterion presents so many difficulties as to render it impracticable for ordinary work. One difficulty lies in the matter of taking records accurately. The rats, after a few trials, run so rapidly that it is extremely difficult for one person to observe and record at the same time. To do this, it is necessary to mark off the maze into small segments and commit to memory some scheme of representation so that records can be jotted down in a purely automatic manner. The work of transcribing this record into distance terms and computing the same is very laborious. *Eliminating these practical difficulties, the distance criterion is in some ways an ideal one.* (italics mine.) There can be no divergence of practice as to what shall be omitted or included and results obtained by different experiments upon the same maze will be strictly comparable." "The distance and error criteria are fundamentally alike. The distance curve is the better representative of the progressive approximation of the act towards automatic accuracy. It portrays all the details of this eliminative process and it approximates the ideal of uniformity and regularity of descent. However, it is impracticable from the standpoint of recording and manipulating the data."

These practical difficulties in "recording and manipulating the data" have been overcome, at least where small animals are the subjects used in the maze. The total distance can be obtained accurately by means of the camera lucida attachment designed by Professor Watson (see Fig. 1) for use with his

[16] Watson, J. B. Noddy and Sooty Terns. *Carnegie Pub.*, no. 103, p. 249, note 1.

[17] Hicks, V. C. The Relative Values of Different Curves in Learning. *Journ. Animal Behav.*, **I**, 138-156.

circular maze. This maze has a wooden base one hundred and fifty centimeters in diameter and six aluminum runways fifteen and five tenths centimeters high and ten centimeters wide. The entrances to the alleys are ten centimeters wide, and are at alternate ends of a quadrant arc. The radial stops in alleys 1 to 5 are also placed at alternate ends of a quadrant arc, the stop in each alley being directly opposite its entrance. Thus, it is possible for a rat to run only one half the circumference of a runway in either direction before being forced to turn. This is not true of alley 6, where no stop is employed. The central circle, or food compartment is twenty centimeters in diameter. A three quarter inch mesh wire top prevents the animals from escaping, without interfering with observations of their movements. The camera lucida attachment consists primarily of two mirrors and an achromatic lens. The arrangement is as follows: A large plate glass mirror is fastened by supporting framework at an angle of forty-five degrees, with its center directly above and one and eight tenths meters from the center of the maze. A somewhat smaller mirror is placed facing the first and making an angle of ninety degrees with it at such a distance away that the light reflected downward falls outside the maze area. In the path of this reflected light is placed a single achromat six centimeters in diameter and of fifty centimeters focus in a mounting provided with rack and pinion adjustment which is fitted into the center of a wooden disc thirty centimeters in diameter. Below this at the focus of the lens is placed a second wooden disc of the same diameter as the first, which serves as a holder for the paper upon which the image of the maze is reflected. Both of these discs are attached to iron collars which slide independently up and down the rod CR, thus making it possible to vary the size of the image. A small curtain of dull black velvet attached to the upper disc serves to exclude all extraneous light from the recording table and as a further aid in sensitizing the eye, a large curtain of dark material encircles the space occupied by lens and recording apparatus as well as the experimenter's chair. This curtain also serves the purpose of completely hiding the experimenter from the animals while they are running in the maze.

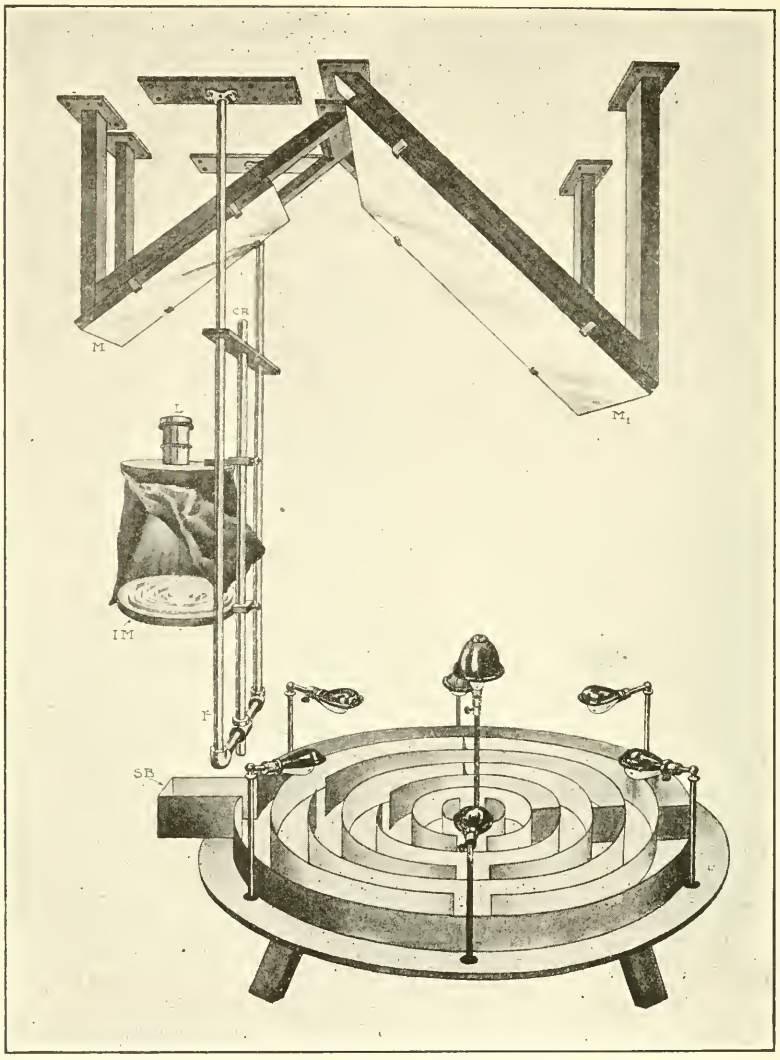

FIGURE 1 Maze with Watson Camera Lucida Attachment

Illumination is obtained by means of six 40-watt tungsten lamps placed symmetrically around the maze and one 150-watt tungsten in the center. These lights are mounted on brass rods and fitted with aluminum shades blackened on the upper

surface. The central shade is circular, those for the peripheral lights are half shades.

The floor of the maze is covered with white linoleum, which can be thoroughly scrubbed whenever necessary. The entrance to the starting box is supplied with a hinged door which can be securely fastened after the animal has been placed inside. The exit is provided with a sliding door which is raised by means of a cord, and closes of its own weight when the tension on the cord is released, thus making it impossible for a rat to return into the starting box after it has once entered the maze.[18]

By means of the two mirrors (M and M'), and the lens (L), an exact image (I M) of the maze is thrown on the recording table where the experimenter can follow every movement of the animal during any passage through the maze. Actual records of these trips are made by tracing on the record sheet with a soft pencil the successive movements of the rat. (See Fig. 2). These tracings, measured with a chartometer shown by calibration to be accurate to within one per cent, form the basis for the distance record. Since the maze is six and four tenths times as large as the image, the distance record obtained in centimeters by the chartometer, must be multiplied by six and four tenths to obtain the actual distance traversed in the maze. For example, if the distance indicated by the chartometer is one hundred and twenty-one centimeters we obtain the actual distance run, thus, 121 centimeters x 6.4 = 774.4 centimeters. The values given in the tables represent the actual distance covered by the rats. Both chart and maze distances were tabulated, and the multiplications made to obtain the latter were checked on the adding machine. In addition to the distance record, such charted pathways also furnish

[18] The maze was used exactly as described above throughout the work in order to maintain the same experimental conditions for all the groups. However, in the course of the experiments, several possible improvements suggested themselves as being desirable:

First.—The maze should be constructed of such material that it could be frequently flushed out with a hose, and a plug should be fitted into the bottom to facilitate cleaning.

Second.—Nitrogen lamps placed at crossfire above the maze would be better than the tungstens surrounding it, and would do away with the shadows caused by the aluminum shades, which, when a very small animal is the subject in the maze, are troublesome.

Third.—A change in the lighting arrangement would make it possible to have the mesh top made in two pieces instead of four, and hinged to the sides of the maze so that it could be lifted easily and noiselessly, thus avoiding frightening the animal within the maze.

accurate account of the excess effort expended, enabling a comparison as to the frequency and extent of the several possible errors as well as a record of the exact steps in their elimination. It can be determined whether a certain error is lessened at each trial and finally disappears, or whether it is dropped out all at once. In short we have an accurate method of tracing the several factors involved in the learning of the maze problem, and a basis for the analysis of the learning process which has heretofore been lacking.

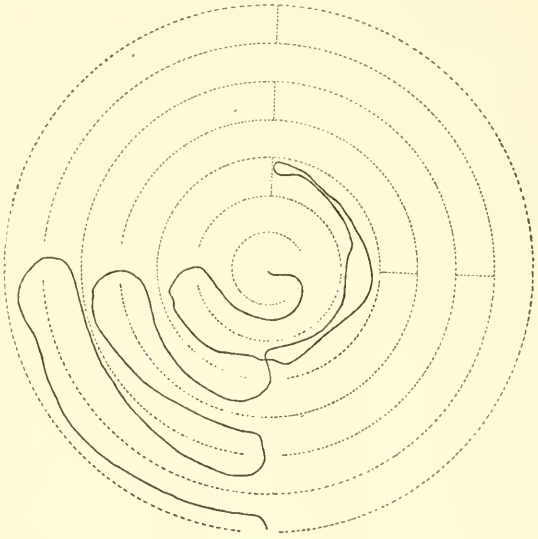

FIGURE 2 Actual Tracing of Pathway Traversed by a Rat in the Maze

The exact method employed in this research concerning the relation of age to the learning ability was as follows:

One week preceding the day on which the animal was to begin work, food was removed from the living cage and the rat was fed each day in the center of the maze which was temporarily partitioned off from the remainder, making it impossible for him to roam at will through the maze. The first day, he was allowed to eat for forty-five minutes; the second day, for thirty minutes; the third day, for twenty minutes. The feeding time was then diminished five minutes on each succeeding day, so

that the day before beginning the problem, the rat had been fed for five minutes in the food box of the maze.[19] Two things were accomplished by this procedure. 1*st*: The rat was rendered quite hungry, a necessary step since food was the stimulus used, but the shock which would have resulted from entire absence of food was avoided. 2*nd*: It became accustomed to some extent to experimental conditions.

On the day when the problem was actually begun, the temporary partition was removed from the maze, a dish of milk-soaked bread placed at the center and the rat put into the starting box (S. B. Fig. 1). The instant it emerged into the maze proper, the door (indicated but not shown in the illustration), was closed behind it, making return into the starting box impossible, the stop watch was started and the tracing begun. Twelve or fourteen minutes might be required to reach the food, and as many as sixteen sheets of paper have been necessary to trace the pathway during a single trial. At the moment of entrance into the food box (F. B.), the watch was stopped, the time noted, and the animal at once removed. This constituted one trip or one trial. The rat was immediately introduced for a second trial, in which the same procedure was followed except that on reaching the food it was allowed to eat for five minutes before being removed. The feeding period was carefully timed with the purpose of keeping the hunger stimulus as uniform as possible. A short ration of grain was thrown into the living cage, and no more food was allowed until the next day's work. Basset[20] had given grain only twice a week, and noted in consequence a disturbance in behavior on the day following that on which grain was given. Ulrich[21] fed his animals in the cage after work, which may account for their slowness in learning the maze as compared with the rats used in this problem.

Two trials were given each day until the problem was learned, i. e., until in six trips made on three consecutive days no error was made from start to finish. In both Basset's and Ulrich's work, a time norm was set, and, although no useless movements were made, unless the act was performed within the limits of

[19] Grain was given in the cage each day at the end of the feeding period.
[20] Basset, G. C. Habit Formation, etc. *Behavior Monograph*, **2** ('14), no. 4.
[21] Ulrich. Behavior Monographs. Vol. II, No. 5.

the time set, it was not considered perfect. For the purposes of this experiment such a norm was not desirable, since one of the points under investigation was the relative final rate of efficiency attainable by rats of different ages. Elimination of all useless movements for three days was therefore considered as sufficient evidence that the problem had been learned. The number of trials required to reach this level of efficiency varied with each rat, the extreme limits being fourteen and one hundred twelve trials. In a single trial any distance greater than four and five tenths meters, which is the length of the errorless pathway from the entrance to the food, represents excess effort on the part of the animal.

If a rat remained in the maze for fifteen minutes without reaching the food box he was taken out and replaced in the entrance box for a second attempt. Distance and time were recorded in the same way as for a successful run, i. e., if the first effort to reach the food proved unavailing after fifteen minutes, and the second attempt was successful after eight minutes, the total time for the first trial would be twenty-three minutes and the total distance the combined distance of the two attempts. Should the rat fail on the second effort also, it was fed for three minutes in the maze with the food box partitioned off as for preliminary feeding, and tried again the following day.

The time and distance records for each trip were carefully tabulated, and form the basis for the conclusions which appear later. In many cases the actual tracings were kept for reference.

EXPERIMENTAL RESULTS

It was planned to work with five groups of rats, twenty-five, sixty-five, two hundred, three hundred and five hundred days old respectively, since it was thought that these ages represented fairly well the successive stages in the growth and development of the animal; twenty-five days for youth, sixty-five days for sexual maturity, two hundred days for maturity, three hundred days for age, and five hundred days for old age. The attempt was made to have thirty rats in each group, but sickness and unavoidable accidents among the animals have brought the number somewhat lower. It has been found extremely difficult to obtain rats for the last group (500 days). Although Slonaker finds the average length of life of the white rat to be thirty-four

months,[22] and Donaldson gives it as three years, from three hundred to four hundred days is the maximum longevity for most of the rats used in this laboratory, and up to this time only twelve have lived to work at the five hundred day age, one of these dying apparently of old age before the problem was learned.[23]

The groups used, with the number of rats in each group were as follows:

Age at which work began	Number of rats in the group
25 days	27
65 "	27
200 "	28
300 "	28
500 "	12

Throughout the experiment two conditions have been rigidly complied with. 1*st*: Every animal was started on the problem upon the exact day at which the proper age was reached. This procedure was followed even when it necessitated starting eighteen rats on the same day, in order that experimental conditions might be kept strictly comparable.

2*nd*: Every animal was run twice every day from its first trial until the last, even though at one stage of the work this required having as many as fifty-eight rats under observation at one time. Such strict continuity of trials precluded the introduction of any factors aside from those involved in the learning process proper. Removal from experimental conditions for even one day would not only cause a change in the physiological tonus of the organism, but would also bring in the matter of retention. So far as was possible the rats were run at the same hour each day, but where large numbers were being used, it was impossible to adhere strictly to this rule, although rats accustomed to run at night did not do so well if used in the mornings and vice versa; this was probably attributable to the acquiring of a certain food rhythm that might not be broken with impunity. Thus it was found that while a difference of an hour in the working time, and hence the feeding time, caused no

[22] Op. cit., *Journ. Animal Behav.*, pp. 37-38, tables.

[23] Dr. Watson has informed the writer that his experience was quite similar, very few of his rats living to be more than 500 to 600 days old.

noticeable change in the behavior of the rats, marked disturbance resulted from a delay of four or five hours.

In general, the behavior of individuals of each group on first entering the maze was the same. The rats showed great hesitancy in leaving the starting box, returned to it frequently after finally entering the maze proper and endeavored to push up the sliding door; they were slow to leave a familiar alley for one unexplored, became excited when a stop was encountered, trying repeatedly to push it aside or to gnaw through the mesh top, and made frequent efforts to escape from the maze. Departure from this type of behavior was noticed among the very old rats and the very young ones. Many of the former evidenced no excitement whatever, often sleeping for several minutes between period of activity, while the latter were far more active than the rats of any other group, and showed no hesitancy in entering unfamiliar portions of the maze.

The time usually decreased very rapidly, the distance less so, during the first three or four trials. For example, on its first trial, rat 34 of the three hundred day group required eleven minutes and forty seconds to reach the food, and the distance covered was forty-nine and six tenths meters. On its second trial, seven minutes six seconds were required, and the distance run was thirty and nine tenths meters; at the fourth trial, success was attained after one minute nineteen seconds, the pathway traversed measuring ten and two tenths meters, while for the sixth trial, the time record was only forty-nine seconds, the distance eight and six tenths meters. By the tenth or fifteenth trial, the decrease in both time and distance had become much more gradual, and continued so until the problem was learned. The rat referred to above, required on the fifteenth trial, twenty-four seconds, and ran seven and two tenths meters; on the thirtieth trial the trip occupied fourteen seconds, and covered five and six tenths meters. This particular animal completed the problem at the sixty-sixth trial, when the time record was seven and two tenths seconds, and the distance record four and five tenths meters, which, it will be remembered, constitutes a perfect run.

The data set forth above may be conveniently tabulated thus:

Trial	Time required to reach food	Distance run in maze before reaching food. (The true pathway 4.5 m.)
1st	11 min. 40 sec.	49.6 meters
2nd	7 " 6 "	30.9 "
4th	1 " 19 "	10.2 "
6th	49 "	8.6 "
15th	24 "	7.2 "
30th	14 "	5.6 "
66th	7.2 "	4.5 "

This rapid decrease in time and distance at the beginning of the problem was characteristic of all groups, and is clearly shown in the initial drop in all the curves.

Twenty-five Day Rats

Work on this group began at twenty-five days when the rats were so small that they could crawl through the mesh top of the maze, and could touch the sides of the alleys only by running from side to side, while other rats could remain in the center of the path and touch both walls of the runways with their vibrissae. These rats were weaned at eighteen days, and were fed in the maze for five days preceding the experiment, the forty-five and thirty minute feeding periods being omitted. For the first day or two after starting the problem, they were allowed to eat for six or seven minutes instead of five minutes at the end of each day's work, since it developed that a shorter ration had a weakening effect on animals so young. The little rats were exceedingly active, and on entering the maze ran so rapidly that it was very difficult, but never impossible, to trace their movements. For the most part they showed great eagerness to escape from the starting box, some even acquiring the habit of lifting the door partway with the nose, and as a rule they had no hesitancy in entering unexplored portions of the maze, in this respect differing from most of the rats in this experiment. The error of circling the food box occurred more often with rats of this group than with those of any other, the explanation being, perhaps, that in their over-eagerness to reach the food they acquired such momentum than they ran past the entrance to the food box.

Twenty-seven rats were experimented with at this age, eleven males and sixteen females, eight strains being represented as follows:

	W M 1/9/14	Y (C F) 2/10/14	G J 3/12/14	A L 3/14/14	X L 3/15/14	K L 9/20/14	X W 11/1/14	Y W 11/1/14	Total
Males...	0	2	1	1	1	3	1	2	11
Females.	2	1	1	1	3	5	3	0	16
All.....	2	3	2	2	4	8	4	2	27

The first letter indicates the father, the second letter the mother, of the litter. Individual rats were distinguishable from each other by a convenient system of ear marks, and on every cage was a tag showing the experimental number, parentage, date of birth, sex and ear mark for each rat contained therein. Thus, W M 1/9/14 R—♀ 4, would be deciphered, rat number four, female, right ear straight, born January ninth, 1914, mother M, father W.

The number of trials required by animals of this group in learning the problem varied from fourteen to fifty-one, the absolute time from four and nine-tenths seconds to nine and one-tenth seconds, the total time from sixty-four minutes to six hundred forty-nine minutes; and the total distance from one hundred thirty-nine meters to four hundred eighteen meters.

The "absolute time" is the average time for the last six trials, represents the limit of efficiency in speed for a given group, and varies among individual rats within the group as well as for the groups themselves. Thus, the record time for the twenty-five day group was made by a rat which could run from entrance to food box in four seconds, but no other rat attained this speed, and one in particular could not make the run in less than eight seconds. The last six trials were all without error and would seem to afford a fair basis for judging the average final efficiency, which for this group was five and seven-tenths seconds. The absolute distance is the same for each group, since the last six trials are errorless, and the true pathway measures approximately four and five-tenths meters.

TABLE I

RUN IN DAY TIME. RAT 15—25 DAYS

Day	Trial	Time	Seconds	Distance		X L 3/15/14 B– male
				Chart	Maze	
1	1	1′ 7.8″	67.8	195	1248.0	Elim. 4-5-6
4/9/14	2		37.2	140	896.0	" 3-4-6-wrong turn-1
2	3		15	106	678.4	" 1-3-4-5
4/10	4		10	70	448.0	Perfect
3	5		25.8	158	1011.2	Elim. 1-2-4-5-6-lost in 3
4/11	6		17.4	120	768.0	" 2-4-5-6-w. t. 1-3
4	7		11.8	72	460.8	" 2-3-4-5-6-w. t. 1
4/12	8		18.6	106	678.4	" 2-3-4-5-6-w. t. 1
5	9		10.4	70	448.0	Perfect
4/3	10		25.8	120	768.0	Elim. 2-4-5-6-w. t. 1-3
6	11		6	70	448.0	Perfect
4/14	12		18	132	844.8	Elim. 3-4-5-6-w. t. 1-2
7	13		5.6	70	448.0	Perfect
4/15	14		6.2	70	44.8	"
8	15		11.8	99	633.6	Elim. 2-3-4-5-6-ret. 1
4/16	16		32.4	129	825.6	" 2-3-4-5-6-ret., w. t. 1
9	17	7.8	5.4	70	448.0	Perfect
4/17	18	7.	7.4	83	531.6	Elim. 2-3-4-5-6-w. t. 1
10	19		5.4	70	448.0	Perfect
4/18	20		5.2	70	448.0	"
11	21		5.4	70	448.0	"
4/19	22		6	72	460.8	Elim. 2-3-4-5-6-too far 1
12	23		5.2	70	448.0	Perfect
4/20	24		9	101	464.4	Elim. 2-3-4-5-6-w. t. 1
13	25		5	70	448.0	Perfect
4/21	26		5.6	70	448.0	"
14	27		7.6	72	460.8	Elim. 1-2-3-4-6-w. t. 5
4/22	28		5.4	70	448.0	Perfect
15	29		5	70	448.0	"
4/23	30		5.2	70	448.0	"
16	31		4.8	70	448.0	" Finished
4/24	32		4.4	70	448.0	" 4/25/14
17	33		4.6	70	448.0	" 34 trials
4/25	34		5.6	70	448.0	"

TABLE II

TWENTY-FIVE DAY RATS

11 males—16 females

Rat	Trials	Time (secs.)	Distance (cm.)	Absolute time (secs.)
4♂	18	2541.4	18035.2	5.7
6	45	1903.4	38666.8	5.1
8	32	959.8	36732.4	5.3
10	28	699.6	23770.4	5.0
15	34	423.2	19424.0	4.9
16	27	1592.2	30016.4	5.5
19	40	721.2	29207.2	5.5
24	24	388.4	16131.6	6.1
27	51	1588.0	41816.0	5.6
36	30	1603.2	28454.4	9.1
37	30	1632.8	23070.4	5.5
1♀	23	2623.2	37819.2	7.2
2	26	562.2	21913.6	5.5
5	14	3897.2	30348.8	5.2
9	38	723.6	27129.6	5.4
11	18	401.4	13900.8	5.7
12	32	2381.0	32648.8	6.1
13	46	999.4	30407.0	4.9
14	24	519.6	18553.6	5.6
17	26	612.8	18771.4	5.6
20	34	945.6	25779.2	5.3
21	36	900.6	26208.0	5.1
22	32	536.2	23979.2	5.4
25	36	597.6	30882.8	5.2
28	28	2643.2	36672.0	5.2
31	44	1838.2	30332.2	5.2
41	17	2082.8	23212.8	7.6
Totals.....	822	36317.8	733383.8	153.5
Averages.....	30	1345.1	27162.3	5.7
Average for ♂....	32	1277.5	27711.3	5.7
Average for ♀....	29	1391.5	26784.9	5.6

The enormous number of figures involved, makes the showing of individual records inexpedient. An exact copy of the daily record for rat number fifteen of the twenty-five day group, from the first to the last trial appears as table I. The averages and totals for each rat which appear in table II are obtained by adding and averaging the daily records for the individual rats. The total time, total distance, total number of trials and the absolute time of the twenty-seven rats were added and averaged to give the average total time, the average total dis-

tance, the average number of trials, and the average absolute time for the group.

The *speed* is the average number of centimeters traveled per second throughout the learning process, and is obtained by dividing the total time into the total distance. For the twenty-five day group these averages were:

	Time			
Trials	Absolute	Total	Distance	Speed
30	5.7 sec.	224 min.	271.6 meters	20.1

The curves shown in fig. 3 are based on the figures in columns 2, 4 and 6 of table II. Only one rat finished in less than fifteen trials, and this is indicated on the first point of the curve. Three rats finished at between fifteen and twenty trials, one at seventeen and two at eighteen trials each, the average being seventeen as indicated on the curve. Between twenty and twenty-five trials three rats finished, at twenty-three, twenty-four and twenty-four trials respectively; the average is twenty-three, and the third point on the curve indicates that three rats finished at twenty-three trials. The same procedure is followed in drawing the time and distance curves except that they are necessarily more condensed. Three rats required approximately four hundred seconds each in which to learn the problem, (numbers 11, 15 and 24), and the first point on the time curve indicates this fact. The fourth point shows that six rats consumed from fifteen thousand to twenty thousand seconds (average for the six, seventeen thousand seconds), in their total number of trials. The fifth point in the distance curve is interpreted to mean that six rats covered between three hundred thousand and three hundred fifty thousand centimeters, (average for the six, one hundred seventy thousand), in learning the maze. It might be well to notice at this point that all of the curves appearing in this paper are constructed on this same plan.

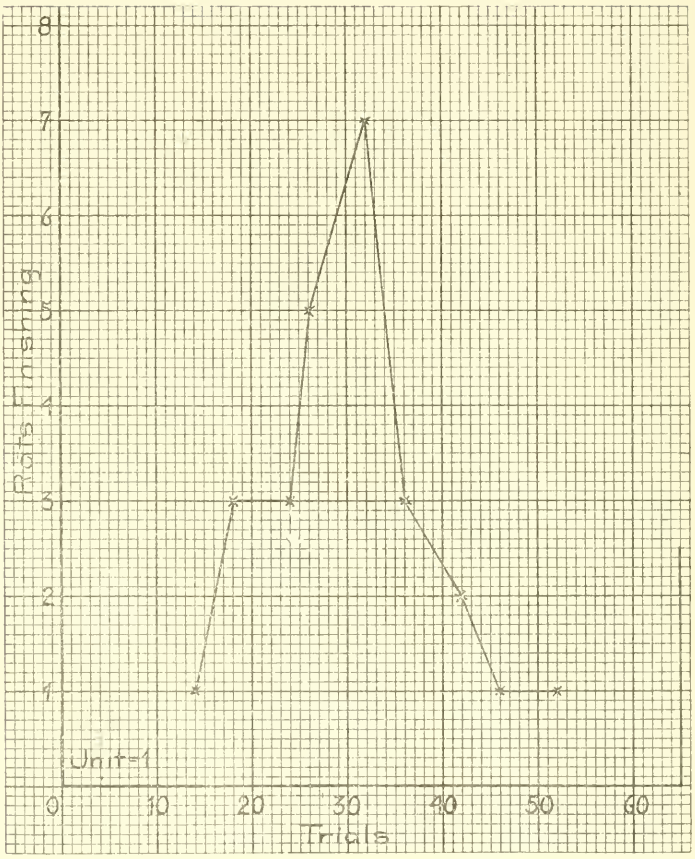

FIGURE 3-A Trial Curve for Twenty-five Day Rats.

The trial curve, (Fig. 3-A) for this group reaches the apex at about thirty, which is the average number of trials for this age.

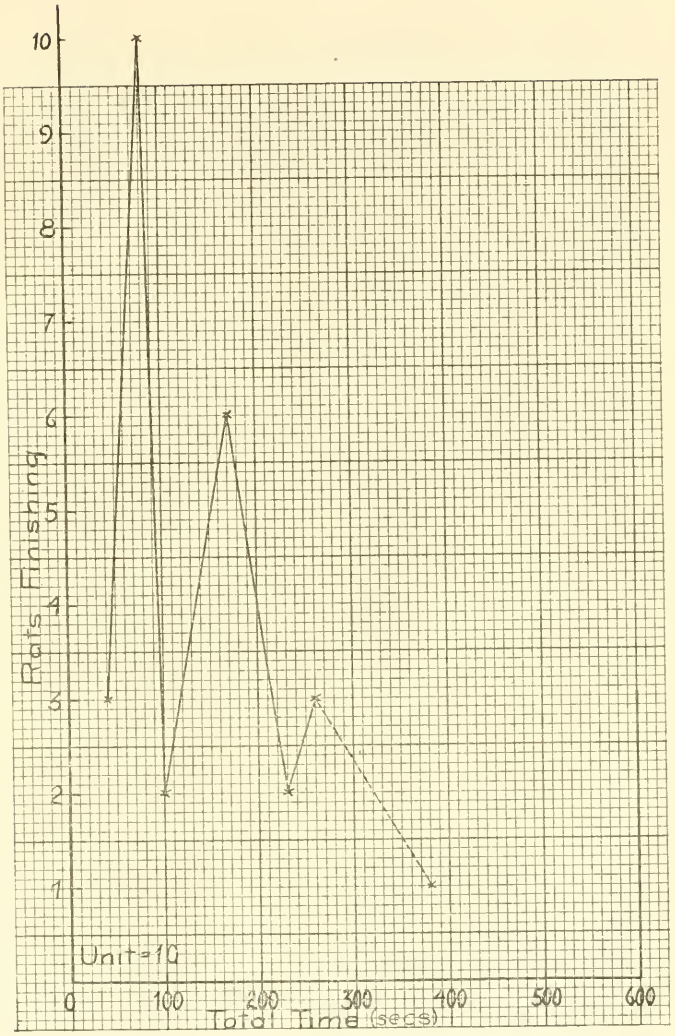

FIGURE 3-B Time Curve for Twenty-five Day Rats.

Two maxima appear in the time curve, (Fig. 3-B) at eight hundred seconds, and at seventeen hundred seconds respectively. A point intermediate between the two would give the time average for the group, approximately thirteen hundred seconds.

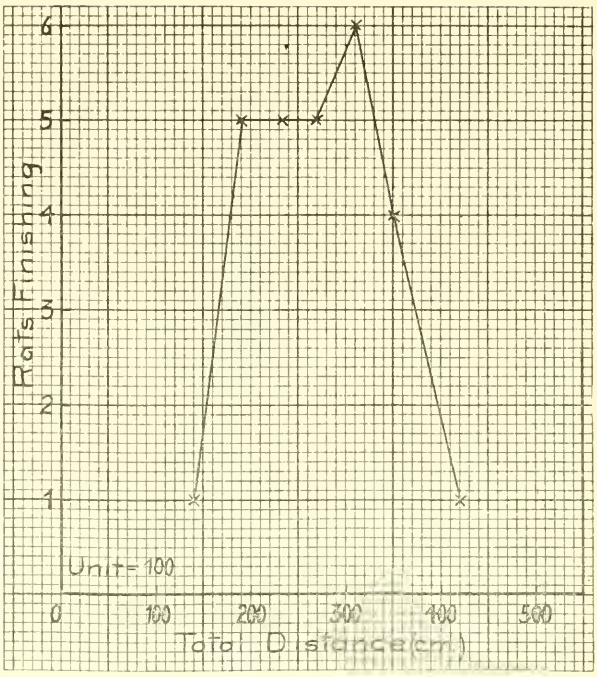

FIGURE 3-C Distance Curve for Twenty-five Day Rats.

The apex of the distance curve, (Fig. 3-C), is at three hundred thousand centimeters, which is not far from the group average of two hundred seventy thousand.

No very close relation seems to obtain between the number of trials required for finishing the problem and the total amount of time or distance. Thus, the rat which finished in fourteen trials had the highest total time record in the group, and a high distance record, while the rat which finished in fifty-one trials had the highest distance record, but a mean time record. The lowest time record was made by a rat finishing in twenty-four trials whose distance record was also low; the lowest distance record, by a rat requiring eighteen trials for the problem, whose time was lower than the average. Except where the time values are very high, time and distance bear a fairly constant

ratio to each other. The exceptions occur when the time record is increased on account of failures (each of which means a count of 900 seconds) in the first part of the learning process. In a trial which has one or more failures as a component part, the distance run is never proportional to the time spent, since if the rat does not refuse to run altogether after several vain efforts to reach the center, it will make frequent halts at the radial stops and at the entrance to the maze. It appears that we have here additional evidence for considering the distance as a more accurate measure of the learning process than the time.

According to the averages which appear below, the females of this group are superior to the males in number of trials required to learn, and in final efficiency (absolute time), but inferior in total time consumed, total distance covered, and speed attained throughout the learning process, so that on the whole, the males may be considered as slightly superior to the females.

	Trials	Time		Distance	Speed
		Absolute	Total		
Males.....	32	5.7 sec.	213 min.	277.1 m.	21.6 cm. per sec.
Females.....	29	5.6 "	232 "	267.8 "	19.2 " " "

Sixty-five Day Rats[24]

These rats began the problem when sixty-five days old, and were fed in the maze for one week before actual experimentation

[24] A group of twenty-seven thirty-five day rats was used early in the experiment, but the results obtained from their records were so at variance with those for other groups that it was felt there must have been some error in the experimental work. Accordingly a control group consisting of thirteen rats was trained near the close of the experiment, with no better results so far as consistency with the main body of averages of the other groups was concerned, but with exactly opposite results from those of the first thirty-five day group. Therefore neither set of figures is given in the body of this paper, since still further work is necessary at that age before any statements can be made regarding it. The averages obtained for the two groups are given below:

	Trials	Time		Distance
		Absolute	Total	
First group.....	63	6.7 sec.	564 min.	552.2 m.
Control group.....	22	7.3 "	160 "	156.7 "

began. They were lively, but did not show the superabundant activity of the twenty-five day group, and were not so speedy. Twenty-seven rats were run, sixteen males and eleven females, representing nine strains as follows:

	E J 11/1/13	E L 11/19/13	E L 12/3/13	W M 1/8/14	Y (C F) 2/10/14	G J 3/12/14	A L 3/14/14	X L 3/15/14	Total
Males..	0	3	2	4	3	0	1	1	16
Females	3	0	0	3	2	1	0	2	11
All.....	3	3	2	7	5	1	1	3	27

Trials varied from fourteen to sixty-five, absolute time from four and seven tenths to eleven and eight tenths seconds, total time from sixty-four minutes to seven hundred thirty-one minutes, and total distance from ninety-one and eight tenths meters to seven hundred fifty meters. Here, as in the preceding group, we can trace no close connection between number of trials and time or distance. The rat which finished in the fewest number of trials had a low time record and the lowest distance record for the group, while the one requiring the greatest number of trials had the highest time and distance records. So far the relation seems very close. But the lowest time record was made by a rat finishing in twenty-two trials, whose distance record was high, while two other rats which finished at twenty-two trials had very high time and distance records. The next to the highest distance record was by a rat finishing in fifty-four trials, while the next to the highest time record was made by one which finished in twenty-two trials. In general, where the trials run very high (65, 54) or very low (14, 16) the distance corresponds rather closely, but for the trials lying between these extremes no such correspondence can be traced. The ratio between time and distance in this group is by no means constant. See table III. The group averages are:

	Time			
Trials	Absolute	Total	Distance	Speed
31	6.8 sec.	219 min.	260.6 m.	19.8 cm. per sec.

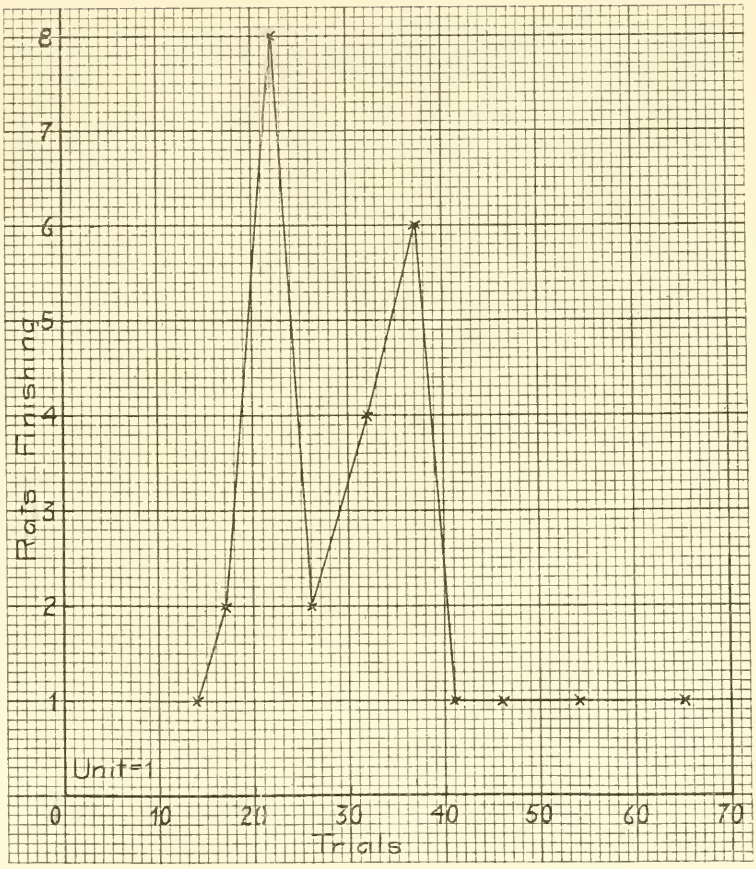

FIGURE 4-A Trial Curve for Sixty-five Day Rats.

There are two maxima in the trial curve for this group, (Fig. 4-A), at twenty-two and thirty-seven respectively, and the group average lies midway between the two, at thirty-one.

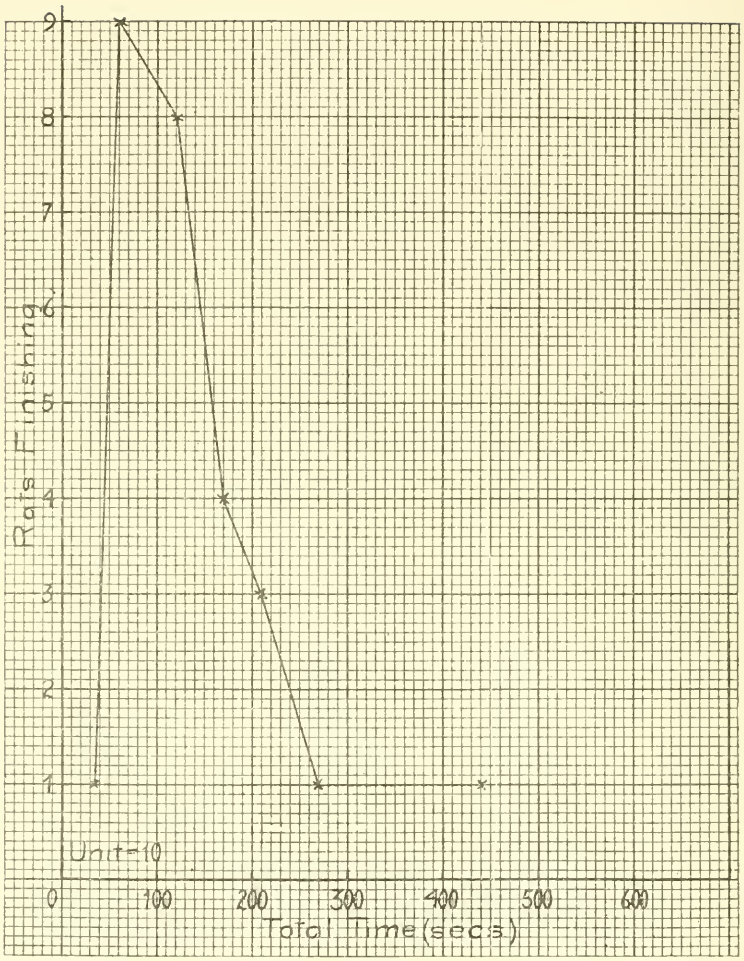

FIGURE 4-B Time curve for Sixty-five Day Rats.

The time curve (Fig. 4-B), shows the highest point at six thousand seconds and one almost as high at twelve hundred seconds, while the average number of seconds for the group was eleven hundred.

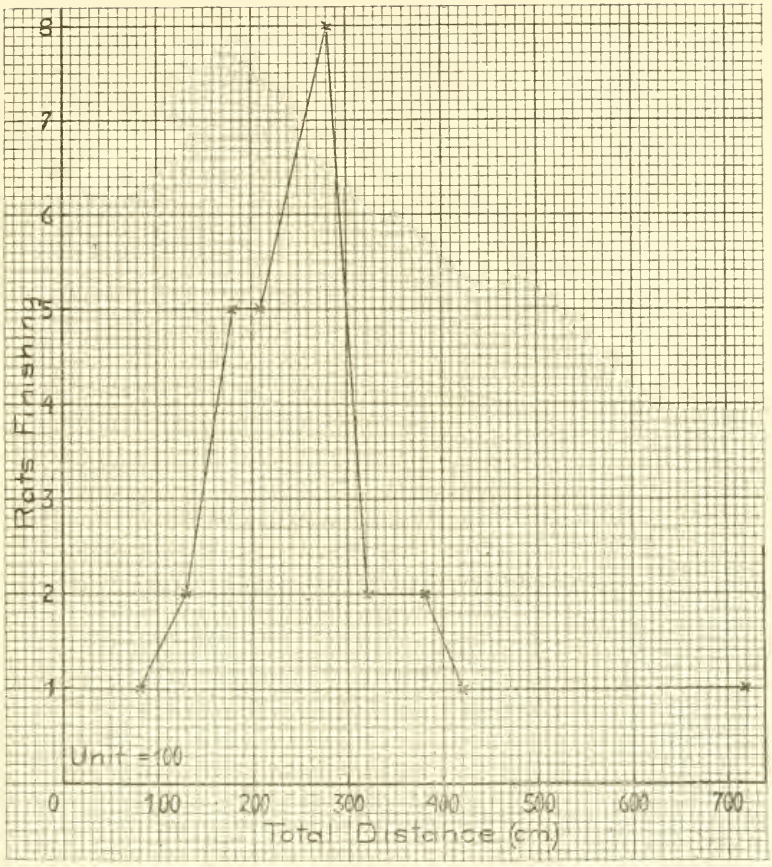

FIGURE 4-C Distance Curve for Sixty-five Day Rats.

For the distance curve, (Fig. 4-C), there is a clearly marked maxima at twenty-six, which corresponds exactly with the group average of twenty-six hundred centimeters.

TABLE III

SIXTY-FIVE DAY RATS

16 males—11 females

Rat	Trials	Time (secs.)	Distance (cm.)	Absolute time (secs.)
26♂	14	454.0	9184.0	11.8
4	16	496.0	11603.2	9.9
5	22	2378.0	26675.2	6.5
6	24	452.4	18182.4	6.3
7	22	645.2	16454.4	6.6
8	30	385.8	19718.4	6.6
9	38	981.4	25117.6	7.2
10	26	519.2	19481.6	5.6
11	18	1184.8	18899.2	5.8
12	34	1724.8	30498.8	6.4
13	22	721.0	16626.4	5.6
14	22	2700.4	29484.8	5.9
20	36	1051.8	29491.2	6.9
21	46	1436.4	33280.0	5.9
22	21	1344.0	21305.6	6.6
24	38	1982.0	39326.0	10.6
1♀	30	1327.4	23033.6	8.2
2	54	1651.8	42368.0	7.9
3	21	1542.0	19603.2	7.1
15	28	2028.0	28921.6	5.5
16	36	1139.4	25177.6	5.3
17	36	454.4	20819.2	4.7
18	65	4388.4	75001.6	5.5
19	32	1312.0	25638.4	4.9
23	38	815.8	28409.6	7.6
27	40	1714.6	35584.0	6.4
28	20	597.4	13849.6	6.7
Totals.....	830	35428.4	703735.2	184.0
Averages.....	30.7	1312.1	26064.2	6.8
Average for♂ ...	26.8	1153.6	22833.0	7.1
Average for♀ ...	36.5	1542.8	30764.2	6.3

Comparison of the male and female records in the group show that while the absolute time of the females is less than that of the males, in other respects the male showing is the better.

	Trials	Time		Distance
		Absolute	Total	
Males.....	27	7.1 sec.	192 min.	228.3 m.
Females.....	36	6.3 "	257 "	307.6 "

Two Hundred Day Rats

These rats were put to work when two-hundred days old, having been fed in the maze for one week preceding the beginning of the problem. They were more erratic than those of any other group used, being jerky and irregular in their movements. Often after making a perfect run in six or seven seconds a rat would drop back to one, two or three minutes with many errors. This behavior was not noted in any other group except in one or two isolated cases.

Twenty-eight rats were used, fifteen males and thirteen females, eight families being represented as follows:

	B D 7/26/13	C C_2 7/30/13	W G 8/24/13	B D 8/28/13	B H 9/18/13	B H 9/25/13	W K 10/2/13	X L 3/15/14	Total
Males..	1	1	3	1	2	3	2	2	15
Females	0	2	2	2	4	2	0	1	13
All.....	1	3	5	3	6	5	2	3	28

Trials varied in number from fourteen to one hundred twelve, absolute time varied from five and two tenths seconds to twenty-four and one tenth seconds, total time from eighty-two to nine-hundred forty-nine minutes, and total distance from one-hundred twenty-five and five tenths meters to nine-hundred and twelve meters. As for previous groups, trials bear little relation to time or distance, but the proportion between total time and total distance is fairly constant. (See Table IV). The lowest

TABLE IV
TWO HUNDRED DAY RATS
15 males—13 females

Rat	Trials	Time (secs.)	Distance (cm.)	Absolute time (secs.)
2♂	81	810.2	51010.4	8.7
5	28	1548.2	21932.4	8.6
8	44	1767.0	30275.2	10.7
9	26	646.3	16672.0	5.7
10	20	496.4	12559.2	6.8
11	20	1548.6	18387.2	7.4
23	30	1191.0	22828.8	7.0
24	30	1564.8	28355.2	7.6
29	104	2636.1	64632.2	17.3
30	107	4735.6	91293.8	24.1
31	64	1877.2	47627.2	6.5
33	32	1343.8	25446.4	8.5
34	22	1084.6	18240.3	10.5
36	32	777.8	18675.2	8.1
38	14	732.8	12569.6	7.9
3♀	26	1280.4	19744.0	9.4
4	112	3014.0	69781.6	7.4
6	18	1825.2	13475.2	8.0
7	54	3851.1	51143.4	7.2
15	32	2409.0	31522.8	5.2
17	56	2995.8	46335.8	6.5
18	79	3663.4	59373.2	6.6
19	49	2031.4	44944.4	5.4
20	27	5694.8	32960.0	8.7
21	32	2140.0	35686.0	6.1
25	35	2990.6	35897.6	7.8
27	22	963.4	19043.6	7.8
35	37	3135.8	45104.8	7.3
Totals.....	1170	36294.9	949517.5	238.8
Averages.....	41.7	2109.1	33911.1	8.5
Average for ♂ ...	39.4	1517.3	29633.6	9.7
Average for ♀ ...	44.5	2791.9	38847.1	7.2

trial record was fourteen, and the time and distance records for the rat making this record were also low. The lowest time record, as well as the lowest distance record was made by a rat finishing in twenty trials. The rat requiring the largest number of trials (112) had a time record but little higher than one which required only thirty-four trials, while its distance record was next to the highest. The highest time record was that of a rat which finished in one hundred seven trials, with the next to the highest distance record, the highest distance record by one finishing in twenty-seven trials, whose time was considerably above the average.

The averages for this group are:

Trials	Time		Distance	Speed
	Absolute	Total		
42	8.6 secs.	351 min.	339.1 m.	16 cm. per sec.

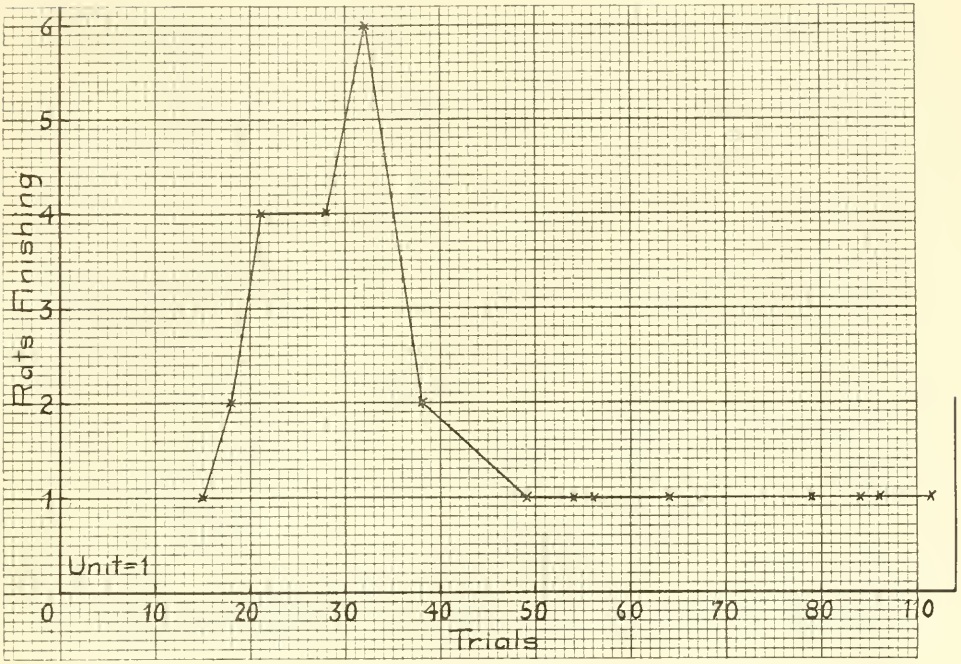

FIGURE 5-A Trial Curve for Two Hundred Day Rats

The apex of the trial curve, Fig. 5-A, lies at thirty-three, while the average for the group is forty-one, but the explanation of the apparent discrepancy is to be found in the records of the six rats who required from fifty-six to one hundred twelve trials to learn the problem, thus running the group average up. No well defined apex can be found in the time curve (Fig. 5-B), the group average, twenty-one hundred seconds showing rather as a depression, nor could it be divined by a glance at the distance curve (Fig. 5-C) that the average lay in the neighborhood of thirty-three thousand centimeters.

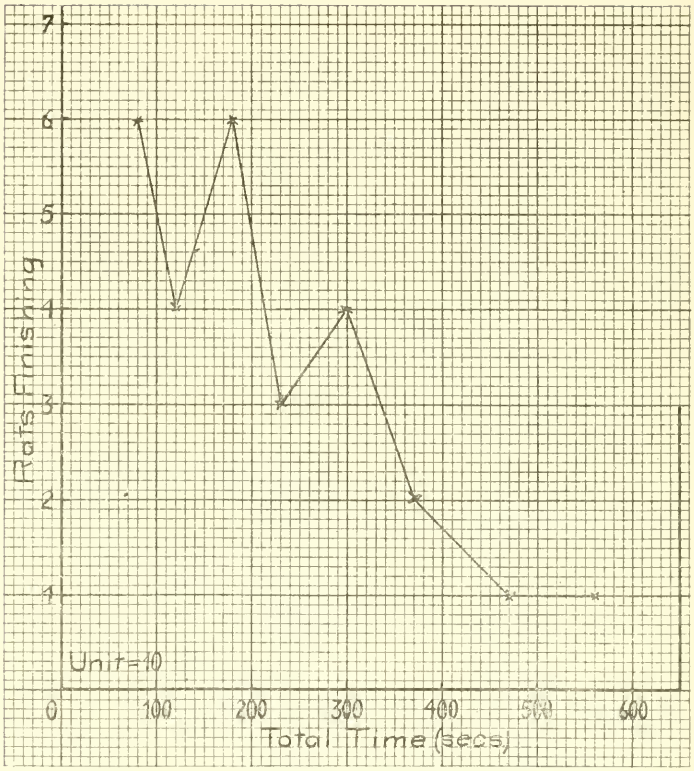

FIGURE 5B Time Curve for Two Hundred Day Rats

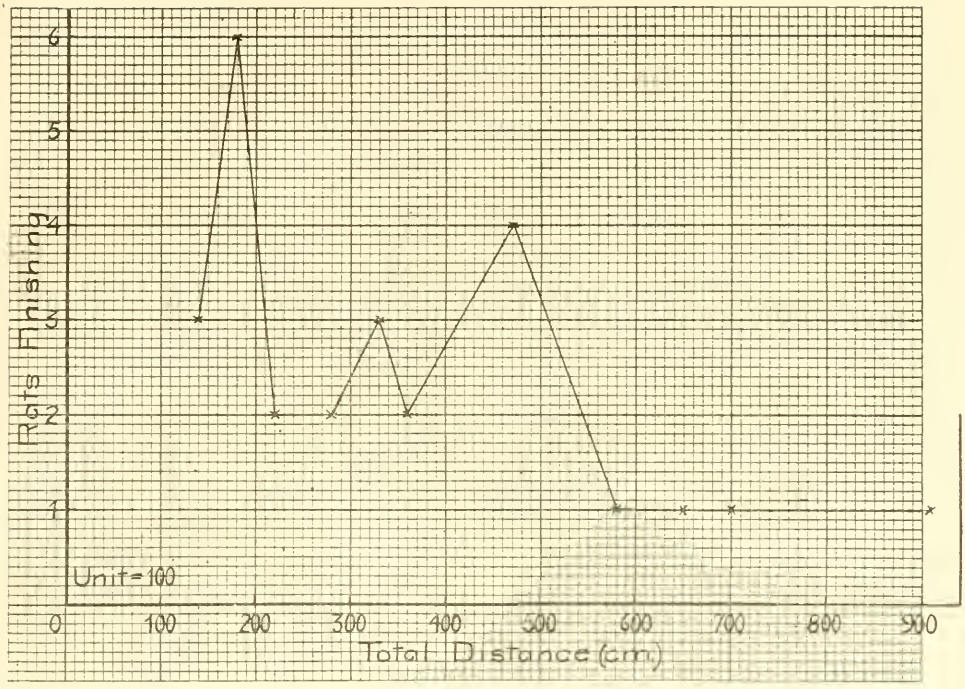

FIGURE 5-C Distance Curve for Two Hundred Day Rats.

If the averages for the males and females be compared, it appears that the former are somewhat superior to the latter except in absolute time. The averages are:

	Trials	Time: Absolute	Time: Total	Distance
Males.....	39	9.7 sec.	219 min.	296.3 m.
Females.....	45	7.2 "	465 "	388.4 "

THREE HUNDRED DAY RATS

This group consisted of twenty-eight rats from ten families, thirteen males and fifteen females, who began the problem when three hundred days old.

	A D	W	B D 5/15/12	W F 5/22/12	B F 5/25/12	W F 6/3/12	B F 6/9/12	C F 6/12/12	B E 6/18/12	C F 6/21/12	Total
Males.....	0	1	0	1	0	4	1	2	2	2	13
Females.....	1	4	1	1	2	2	2	1	1	0	15
All.....	1	5	1	2	2	6	3	3	3	2	28

They were fed in the maze for ten days before the beginning of the problem, preliminary feeding being thus extended because it was found that rats so old contracted digestive troubles unless the decrease in food supply was made more gradual than for the younger animals. They were allowed to eat for from six to eight minutes instead of five, at the close of each day's work, since they ate much slower than the younger rats, and could not obtain sufficient nourishment in the shorter time. These rats differed markedly in behavior from those in any of the preceding groups in that they were lethargic, inactive, and often went to sleep in the maze instead of working at their problem. A few of the animals of this group were from The Wistar Institute, and were somewhat timid and difficult to handle, but even among animals bred in this laboratory the same disinclination to work was noted, although with our own rats it did not last so long. When the rats finally began to work, they went about it differently from those of other groups. They were very deliberate, followed the culs de sac out to the bitter end whereas the other rats often turned back toward the true path before reaching the alley stop; furthermore, they did not hesitate to enter the unexplored runways as did most of the other rats, in this last respect resembling the twenty-five day rats.

The trials varied from fourteen to eighty-four, absolute time from five and eight tenths seconds to thirty-five and two tenths seconds, total time from one hundred nine minutes to two thousand two hundred seventy-four minutes, and total distance from one hundred seventeen and three tenths meters to six hundred nine and six tenths meters.

TABLE V

THREE HUNDRED DAY RATS

13 males—15 females

Rat	Trials	Time (secs.)	Distance (cm.)	Absolute time (secs.)
5 ♂	29	8221.0	30962.8	12.7
17	48	1943.8	35558.4	9.5
22	82	1222.06	72300.4	14.5
24	42	5605.8	43411.2	13.5
25	54	1862.4	33017.6	10.2
26	19	8937.2	33760.0	11.3
30	27	3167.4	19980.8	13.1
33	16	956.4	11731.2	19.5
34	66	2899.0	55929.6	10.7
36	26	2618.4	24358.4	8.6
37	44	2861.2	33068.8	13.0
38	34	1706.6	28793.6	6.5
39	35	4227.2	35916.8	6.9
1 ♀	20	11113.2	31609.6	35.2
2	62	4745.6	56636.4	5.8
9	25	7029.6	34898.6	9.0
12	24	13639.6	32563.0	7.4
14	19	4320.6	26163.2	21.4
15	78	2646.4	50601.8	11.5
16	20	1640.2	18227.2	14.3
18	40	2431.4	42406.4	6.6
19	14	1598.4	15059.2	13.0
20	58	4614.9	66342.4	15.5
21	30	1682.6	26035.2	8.3
27	70	7567.6	60960.0	6.2
28	38	1134.0	24652.8	6.2
31	84	2087.0	56115.2	8.3
35	38	1438.4	28076.8	6.7
Totals.....	1142	124916.5	1029137.4	325.4
Averages.....	40.7	4461.3	36754.9	11.6
Average for ♂ ...	40.4	4402.0	34437.6	11.5
Average for ♀ ...	41.3	4512.6	38023.1	11.7

No connection between number of trials and time or distance was found, and the ratio of total time to total distance did not appear to be constant. (Table V). Thus, the lowest number of trials (14) was made by a rat whose total distance was next to the lowest, but whose time record was higher than that of one rat finishing at sixteen trials and those of two rats finishing at thirty-eight trials each. The greatest number of trials (84) was made by a rat whose time record was exceeded by eighteen others, while its distance record was exceeded by four

others. The lowest time record as well as the lowest distance record was made by a rat which finished in sixteen trials, the highest time record by one requiring twenty-four trials, whose distance record was an average one. The highest distance record belongs to the rat with next to the highest number of trials whose time is also next to the greatest.

Group averages were:

	Time			
Trials	Absolute	Total	Distance	Speed
41	11.6 sec.	743 min.	367.5 m.	8.2 cm. per sec.

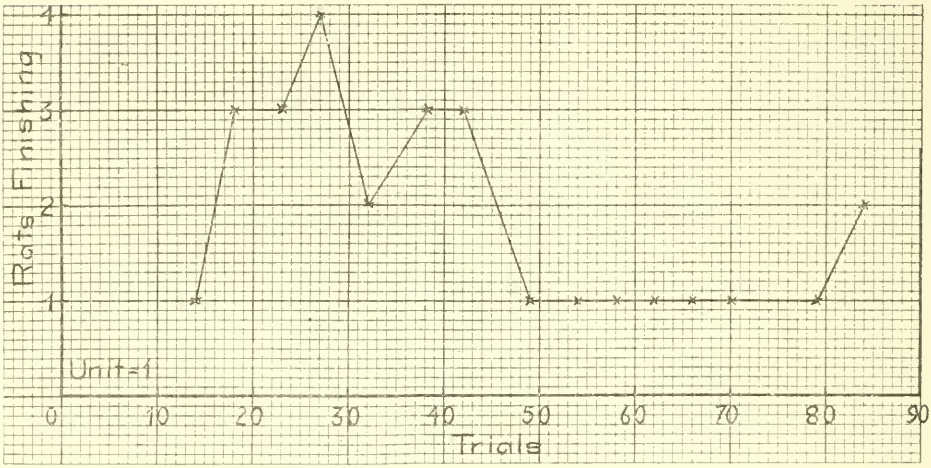

FIGURE 6-A Trial Curve for Three Hundred Day Rats

The apex of the time curve (Fig. 6-A) lies at twenty-eight, although the average is forty-one. The large number of rats finishing after thirty trials however easily accounts for the apparent discrepancy. Two maxima are found in the time curve, (Fig. 6-B), at seventeen hundred and twenty-eight hundred respectively, but again the general average is raised by the twelve animals who required more than three thousand seconds in which to learn the maze pathway. There is a decided apex in the distance curve (Fig. 6-C) at thirty-three thousand, which nearly corresponds with the group average of thirty-six thousand.

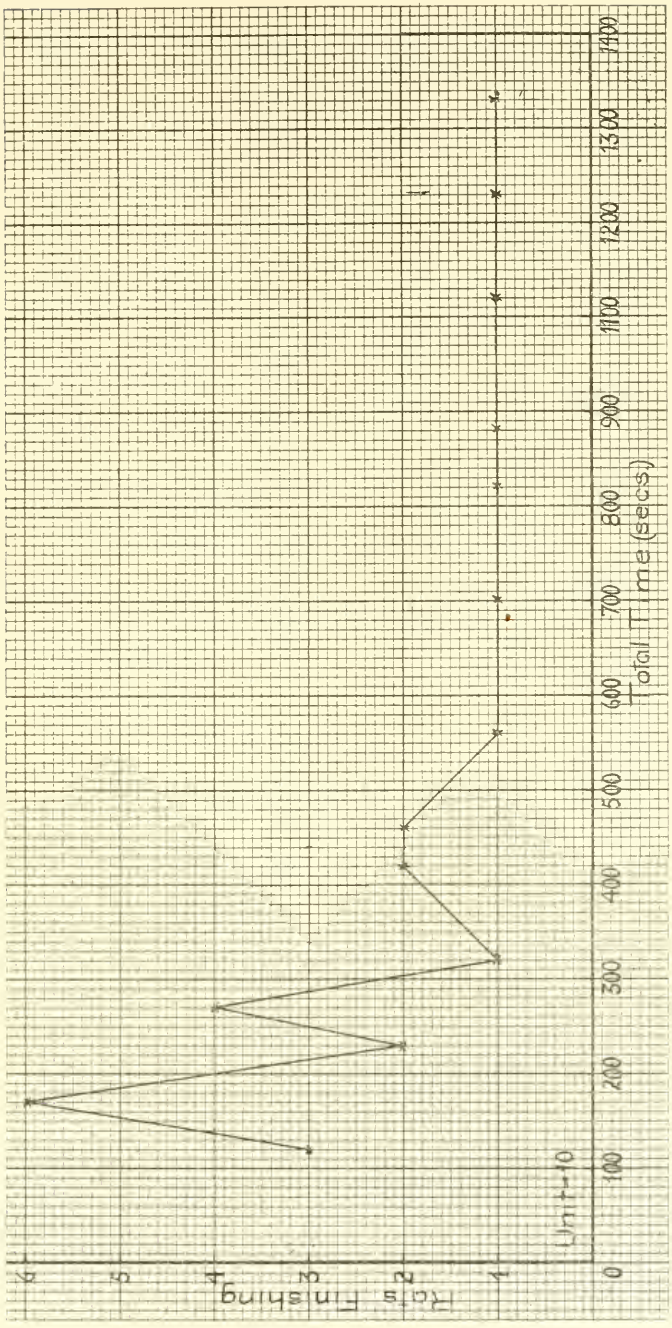

FIGURE 6-B Time Curve for Three Hundred Day Rats

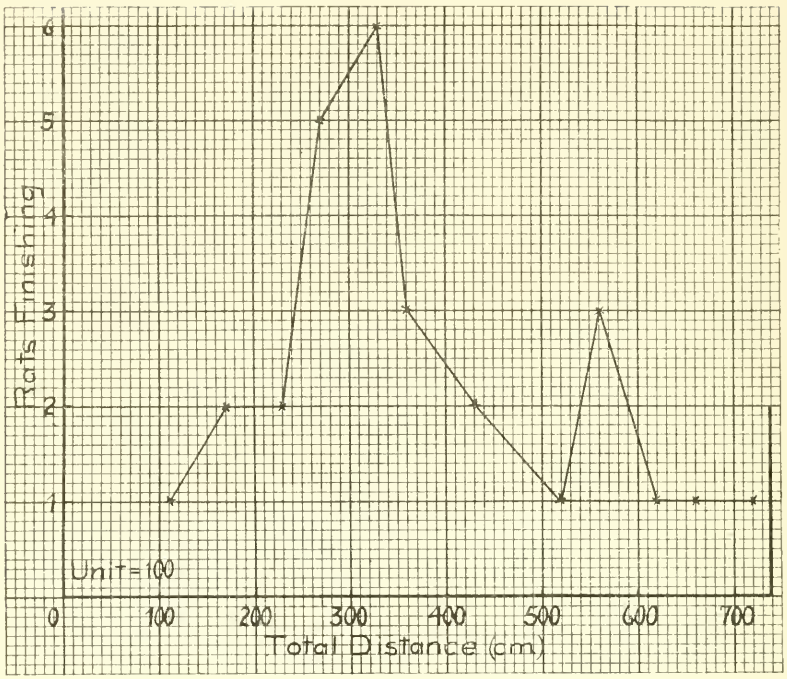

FIGURE 6-C Distance Curve for Three Hundred Day Rats

There was almost no difference between the learning of the males and that of the females, such difference being even less marked than in the twenty-five day group. The averages are:

	Trials	Time		Distance
		Absolute	Total	
Males.....	40	11.5 sec.	733 min.	344.8 m.
Females.....	41	11.7 "	752 "	380.2 "

FIVE HUNDRED DAY RATS

Records on only eleven rats have been obtained in this group, six strains being represented by four males and seven females who began the problem when five hundred days old. These rats like the 300-day animals, were fed in the maze for ten days previous to the commencement of the problem, and allowed to eat for from six to eight minutes at the close of each day's work

on account of their age. Little difference in behavior was noted between them and the three hundred day animals.

	C F 6/12/1	B E 6/18/13	C F 6/21/13	B H 9/25/13	W K 10/7/13	E J 11/11/13	Total
Males.....	0	0	1	0	3	0	4
Females...	1	1	2	1	0	2	7
All.....	1	1	3	1	3	1	11

Too few rats have been used to make this group really comparable with the rest, but averages and totals are nevertheless shown. Trials varied in number from eighteen to fifty-six, absolute time from five and four tenths seconds to seventeen and eight tenths seconds, total time from one hundred and sixty-seven minutes to eleven hundred and sixty-seven minutes, and total distance from one hundred seventy-two and eight tenths meters to six hundred forty-one and six tenths meters.

Again there is no relation apparent between trials and time or distance, and total time and total distance do not bear a proportional relation to each other. (Table VI). The rat which

TABLE VI

FIVE HUNDRED DAY RATS

4 males—7 females

Rat	Trials	Time (secs.)	Distance (cm.)	Absolute time (secs.)
6♂	32	1331.8	20922.2	16.2
9	43	7004.0	42167.8	7.6
10	18	6021.8	17280.0	17.8
11	56	2877.6	64167.4	5.4
1♀	47	3117.3	41450.4	13.2
2	34	1570.2	27381.6	9.4
3	45	3763.8	37791.2	10.5
4	38	3124.2	30559.6	7.5
7	32	1005.2	20577.0	8.7
12	38	3534.0	46364.4	9.9
14	30	3443.0	30616.8	11.7
Totals.....	413	36792.9	379278.4	117.9
Averages.....	38	3344.9	34479.9	10.7
Average for♂ ...	38	4326.3	36134.3	11.8
Average for♀ ...	37	2794.0	33534.4	10.1

finished in the fewest trials (18) had next to the highest time record but the lowest distance record, while the rat requiring the greatest number of trials (56) had a time record lower than the average, with the highest distance record. The highest time record was made by a rat which finished in forty-three trials, whose distance record was excelled by one other, the lowest, by one which finished in thirty-two trials with a distance record next to the lowest.

Group averages are:

	Time			
Trials	Absolute	Total	Distance	Speed
38	10.7 sec.	557 min.	332.9 m.	9.9 cm. per sec.

Discussion of the curves seems hardly worth while in view of the small number of results on which they are based. The average number of trials lies between the two apices of the trial curve, (Fig. 7-A), the average amount of total time required, thirty-three hundred seconds, agrees with the second maximum in the time curve, (Fig. 7-B), and the distance average lies at the middle one of the three maxima of the distance curve. (Fig. 7-C).

FIGURE 7-A Trial Curve for Five Hundred Day Rats

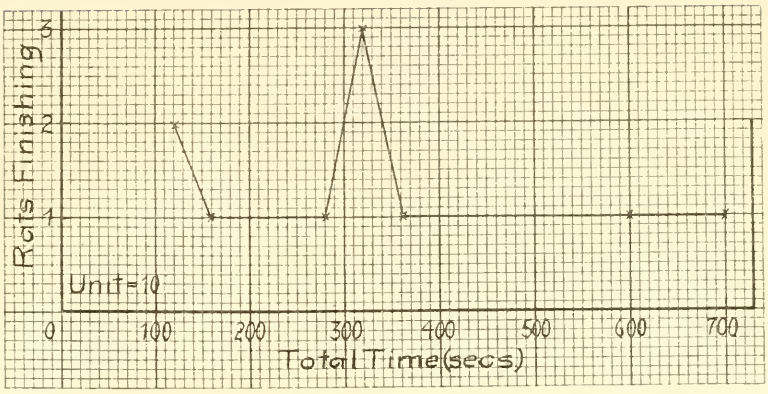

FIGURE 7-B Time Curve for Five Hundred Day Rats

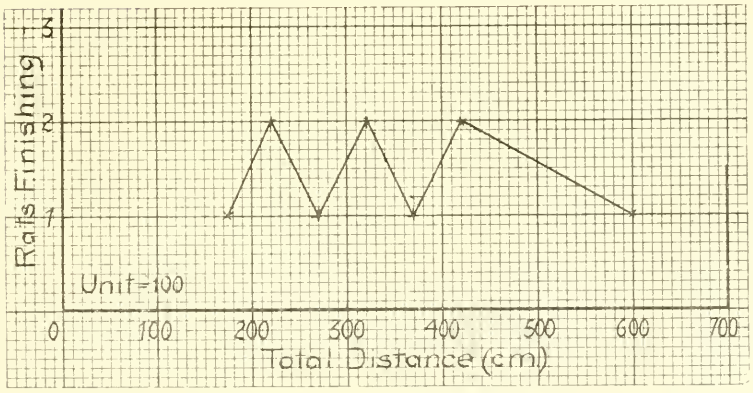

FIGURE 7-C Distance Curve for Five Hundred Day Rats

The males appear to be inferior to the females as is shown by comparing the averages for the two sexes:

	Trials[25]	Time: Absolute	Time: Total	Distance
Males.....	38	11.8 sec.	721 min.	361.3 m.
Females.....	37	10.1 "	466 "	313.9 "

COMPARISON OF RESULTS OBTAINED FOR THE DIFFERENT AGES

TABLE VII

GENERAL AVERAGES

Age	Trials	Time: Absolute	Time: Total	Distance	Speed
25 days:					
Males ...	32	5.7 sec.	213 min.	277.6 m.	21.6 cm. per second
Females .	29	5.6 "	232 "	267.8 "	19.2 " " "
All	30	5.7 "	224 "	271.6 "	20.1 " " "
65 days:					
Males ...	27	7.1 "	192 "	228.3 "	19.8 " " "
Females .	37	6.3 "	257 "	307.6 "	19.9 " " "
All	31	6.8 "	219 "	260.6 "	19.8 " " "
200 days:					
Males ...	39	9.7 "	263 "	296.3 "	19.5 " " "
Females .	45	7.2 "	465 "	388.4 "	13.8 " " "
All	42	8.6 "	351 "	339.1 "	16.0 " " "
300 days:					
Males ...	40	11.5 "	734 "	344.3 "	7.8 " " "
Females .	41	11.7 "	752 "	380.2 "	8.2 " " "
All	41	11.6 "	743 "	367.5 "	8.2 " " "

Table VII shows the general averages for each age as well as those for the males and females separately. Averages for the five hundred day rats are omitted for reasons already given.

[25] Two five hundred day rats did not finish the problem. Each had been given sixty trials when the experiment had to be discontinued. The records were:—

	Total time	Total distance
Rat no. 15.....	13046.2 sec.	58371.2 m.
" " 16.....	17400.2 "	64174.4 "

Rat number 15 had made five perfect runs in the course of his training, but had never gone perfect twice in succession.

Rat number 16 had made twelve perfect runs, and on two occasions had traversed the path twice in succession without error, but never succeeded in doing it three times in succession.

The time record for each rat is greatly above that of any rat shown in table VI. The distance record of rat 16 is slightly above the maximum for the five hundred day rats as recorded in the table, and that of rat 15 nearly reaches it. Had it been possible to continue the experiment until both of these animals learned the problem, the group averages for the five hundred day rats would have been considerably increased.

The number of trials required by the rats in order to learn the maze increases with age except in the case of the three hundred day group where the average is very slightly below that of the two hundred day group.

The sixty-five day males learned the problem in fewer trials than the twenty-five day ones but the females of the older group required more trials than those of the younger. There is rather a sharp dividing line between the young animals (25 and 65 days) and the old animals (200 and 300-days) the former acquiring the maze habit with considerably fewer trials than the latter.

The total time consumed in perfecting the habit also shows a regular increase with age except for the sixty-five day rats whose time record is slightly below that of the twenty-five day ones. The apparent superiority of the older group over the younger is attributable solely to the record made by the males, since the females at sixty-five days have a higher record than those at twenty-five days. Again we see that the two younger groups are quite distinct from the two older ones, requiring considerably less total time in which to learn the problem. The high average of the three hundred day group is due in part to the large number of failures which occurred in early trials at that age, but is also partly attributable to their slower bodily movements.

Total distance shows a regular increase with increasing age except for the sixty-five day group, where again the lowering of the average is due to the superiority of males over those of the twenty-five day group, the females being superior in the twenty-five day group as compared with the sixty-five day group. The difference between the two younger groups and the two older ones is not so marked as that for trials or time, but it is nevertheless apparent that the members of the latter covered more ground than those of the former. Yerkes found his older dancers somewhat superior to the younger ones in learning the labyrinth. The writer finds that the younger rats learn the maze in fewer trials, that their absolute time is less, their total time and distance are less, and that their speed is greater than in the case of the older rats. His ten month dancers were superior to those of one to two months while the twenty-five and sixty-five day rats of this experiment form

the maze habit more quickly than those three hundred days old.

The speed (which it will be remembered is the average number of centimeters run per second throughout the learning process, no distinction being made between early and late trials) without exception decreases with increased age. The last group is distinctly slower than any other and this to our mind is again proof of the lessening of activity with age.

The absolute time, which we have taken as the indication of final efficiency, also diminished with increasing age, and is considerably lower at three hundred days than at any previous age. Thus, while at twenty-five days the average length of time required for the execution of a perfect run was five and seven tenths seconds, at three hundred days the very best time in which the food could be reached was eleven and six tenths seconds, more than twice the time of the youngest group. It follows that in the formation of habits in which the factor of speed is of an importance, equal to or greater than that of exactness, the older animals would be considerably handicapped. In the field of animal experimentation illustrations of habits where speed is an important factor are difficult to find. On the human side such an illustration might be had in the acquiring of technique in piano playing or voice culture, either of which demands the rapid succession of the muscular activities involved in rendering scales, arpeggios, trills, etc. It would seem that habits requiring extreme rapidity of execution within a prescribed rhythm could not be learned by the older animals.

A comparison of the relation of distance to time in the younger and older groups is interesting. In the first two groups the distance is relatively high showing the excess activity displayed by the younger animals, in the two hundred day group it is about the same as the time, indicating that excess activity is at a minimum, while in the last group it is much less than the time, showing that the effects of old age have begun to manifest themselves through a general slowing up of activity.

If the distance alone be taken as a measure of activity, our results agree with those of Slonaker who found the most active age to be between ten and twelve and a half months, since our three hundred day rats covered more distance in learning the problem than any other group. If, however, distance be con-

sidered in relation to time, the older rats appear much less active than the younger ones, as is shown by the average high speed attained by the young in comparison with the old. Certainly the behavior of the old animals when in the maze is much more deliberate than that of the young ones, and the writer believes that if Slonaker had possessed some means of measuring the amount of activity per unit of time he would have found the young far more active than the old.

In Table VIII is given the average speed for each group, for the first, second and tenth trials, the two trials immediately preceding the last six, and the last six trials. The increase of speed from the first to the second trial is considerable except in the two hundred day group where there is a decided decrease. The gain from the second to the tenth trial is great except for the three hundred day group where it is comparatively small. From the tenth trial to the two preceding the last six the gain for the two hundred and three hundred day groups is greater than for the twenty-five or sixty-five day groups, and from these two trials to the last six trials the gain is again greater for the three hundred day rats. This gives a slight indication as to where the most rapid learning occurs. A full set of tables showing the speed for every trial of each group would be necessary for an adequate discussion of the question, but from the present incomplete data it appears that the learning in the two younger groups is most rapid during the early stages, while for the older groups it is more rapid during the later trials. Especially is this true of the three hundred day group, the increase in speed being very gradual during the first ten trials then more than doubling from the tenth to the two immediately preceding the last six. In general, speed, for the separate trials tabulated, decreased with age, which accords with our observation on the average speed for each group during the entire period of formation of the maze habit.

TABLE VIII

SPEED—CENTIMETERS PER SECOND

Age	First trial	Second trial	Tenth trial	Two preceding last six	Last six
25 days	7.8	9.8	52.0	74.2	90.0
65 "	7.2	11.1	35.0	52.0	75.0
200 "	8.3	4.7	21.0	40.7	56.2
300 "	3.0	4.2	9.5	20.8	40.9

INCIDENTAL TESTS

Although the primary object of this investigation was to determine the relation of age to rapidity of habit formation, several minor points of interest have been touched upon in the course of the experimentation which it may be well to mention.

Effect of Sex on Rapidity of Learning

The ideal way in which to test this matter would be to have an equal number of males and females from each litter used, and at least twenty animals of each sex used at each age. In our work this was impossible, but the averages given in Table IX are in no instance based on less than eleven animals, the number in each case being given.

TABLE IX

Age	Trials	Time: Absolute	Time: Total	Distance	Speed
25 days:					
11 Males....	32	5.7 sec.	213 min.	277.1 m.	21.6 cm. per sec.
16 Females..	29	5.6 "	232 "	267.8 "	19.2 " " "
65 days:					
16 Males....	27	7.1 "	192 "	228.3 "	19.8 " " "
11 Females..	37	6.3 "	257 "	307.6 "	19.9 " " "
200 days:					
15 Males....	39	9.7 "	263 "	296.3 "	19.5 " " "
13 Females..	45	7.2 "	465 "	388.4 "	13.8 " " "
300 days:					
13 Males....	40	11.5 "	734 "	344.3 "	7.8 " " "
15 Females..	41	11.7 "	752 "	380.2 "	8.2 " " "
Gen. Av.:					
55 Males....	35	8.2 "	351 "	286.5 "	17.2 " " "
55 Females..	38	7.7 "	427 "	336.0 "	15.3 " " "

It may be seen from the table that the males are at every age somewhat superior to the females in learning ability, their superiority being less marked in the young and old groups (25 and 300 days) than in the two intermediate groups (65 and 200 days). The general averages for an equal number of males and females show the males superior to the females in all points save one, that of absolute time. They finished in fewer trials, required less total time, and covered a smaller amount of dis-

tance in learning the problem than did the females, while their speed was slightly higher. This conclusion is at variance with that of Yerkes regarding the dancer, he having found the females superior to the males in learning the labyrinth. In the matter of final efficiency as evinced by the absolute time, the females are superior to the males at all ages except three hundred days when the two records are practically equal. The general average shows this to be the one point wherein the record for the females is better than that for the males.

The mean variation from the time average is less for the males at all ages, their distance variation is less at sixty-five and three hundred days. The general average shows the smallest time variation for the males and the smallest distance variation for the females. These results do not agree with those of Yerkes on the dancer. His ten month (300 day) dancers learned the labyrinth more rapidly, the number of trials required being the measure of learning, than those one to two months old (30-60 days) while there was no difference in the learning ability of the females at the two ages. My twenty-five and sixty-five day rats of both sexes formed the maze habit considerably more rapidly than the three hundred day animals.

The fact that in the number of trials, total time and total distance required to learn the problem, the males at sixty-five days are superior to those at twenty-five days while the reverse is true of the females, suggests the possibility that the capacity for habit formation develops earlier in the females than in the males.

Day and Night Work

It has been stated by Slonaker, and is generally believed, that the albino rat is nocturnal. With a view to testing this matter certain rats in the twenty-five and two hundred day groups were run always in the day time, certain others always at night. The averages for the day and night rats were obtained in the same manner as the group averages, from Tables A and C.

The twenty-five day rats run during the day were numbers 1, 2, 4, 5, 6, 15, 16, 17, 19, 20, 21 and 24, seven of which were males and six females. Those run at night were numbers, 8, 9, 10, 11, 12, 13 and 14, two males and five females. The

averages which appear below in Table X seem to show the day rats slightly superior in distance and trials while the night rats consumed less time and had a slightly higher final efficiency. These differences are negligible, and there may be said to be no difference in learning at this age between the rats run in the day-time and those run at night.

The day group of two hundred day rats consisted of two males and four females numbers 18, 19, 20, 21, 23 and 24, while the night group included two males and four females numbered 6, 7, 8, 9, 10, 15 and 17. The averages show the night group to be superior to the day group in every respect save that of final efficiency. Nevertheless, we are inclined to hold to our previous statement that no difference is shown in learning ability, for the following reason: The general average for the females of this group was considerably higher than that for the males except in the matter of absolute time. In the day group there were only two males and four females. Were the number of males the same as the number of females it is our belief that the average would be considerably lowered and the day and night groups prove to have practically the same ability in learning the maze problem.

TABLE X

AVERAGES

	Trials	Time		Distance
		Absolute	Total	
25 days:				
Day.....	29	5.5 sec.	207 min.	247.4 m.
Night.....	31	5.4 "	159 "	261.6 "
200 days:				
Day.....	41	6.2 "	461 "	373.5 "
Night.....	34	7.2 "	325 "	267.9 "

CONTINUATION OF WORK AFTER THE PROBLEM HAS BEEN LEARNED

Another question which interested us, was, what would be the effect on efficiency if rats which had learned the problem were caused to continue their runs in the maze for a long period, i. e., would continued practice cause a marked increase in efficiency evinced by a lowering of the absolute time record, or had the highest possible level already been reached in the last six trials of the learning process ?

To test the matter, six rats of the sixty-five day group were kept at work for more than one hundred sixty trials after learning was complete, the average time for each six trials was computed and appears in Table XI as twenty-seven tests. Taken

TABLE XI

Ab. T.	Rat 8 6.6	Rat 12 6.4	Rat 14 5.9	Rat 15 5.5	Rat 16 5.3	Rat 17 4.5
1	6.7	7.1	6.5 ee	5.1†	5.3	4.3 e†
2	6.4†	7.0	5.3 e†	4.8 e†	4.7†	4.5
3	5.4†	7.9 ee	4.8†	4.8 e†	6.9 e	6.3 e
4	5.2*	9.4 ee	6.1 e	4.1*	9.1	6.0 e
5	6.7 e	17.6 eee	5.8†	4.6†	6.1	5.1
6	5.8†	8.5	5.1†	4.9†	8.8	10.8 ee
7	7.2 e	6.8 e	5.8†	7.8	6.2	4.8 ee
8	5.8†	5.4 e*	5.0 e†	5.4†	7.9 e	4.7
9	5.5 e†	6.4	5.0*	6.4 e	6.1	4.3*
10	8.2 eee	5.4†	5.8†	4.3†	14.6 eeee	4.7
11	5.2†	6.1 e†	5.5†	14.3 e	5.6 e	6.6 e
12	6.9 eeeee	5.5†	4.6†	4.7†	6.5 e	4.9 e
13	6.7 e	5.7 e†	4.8†	8.2 e	6.9 e	4.4†
14	21.5 eee	7.2 e	5.2†	8.1	6.2	4.7
Errors	15	13	5	5	9	8
15	6.8 ee	8.8	4.9†	5.0†	5.1*	4.7
16	10.0 eee	6.7 e	5.7 e†	19.3 ee	8.4	5.4
17	14.1 eeee	6.2†	4.7 e†	5.6 e	8.2 ee	5.1
18	6.9 ee	5.7†	5.4†	5.1†	6.7	4.8
19	10.3 ee	9.9	5.2†	5.5	7.3	5.4
20	5.9 e†	12.2	5.2†	5.1†	5.7	4.5
21	19.0 eeeee	7.5	5.8†	7.7	22.0 e	8.6 ee
22	11.4 eeeee	9.3 e	8.8	5.6	24.3 eeee	7.6 eeee
23	6.5 e†	7.9 e	5.7 e†	12.2	14.5 e	19.9 eeee
24	10.1 eee	9.8 eee	6.4	7.1	16.1 eeeee	6.6 e
25	6.9 ee	12.9 ee	6.0	5.4†	8.9 e	5.4
26	11.5 eeeee	6.0†	5.4 e†	5.7	10.4 e	6.7 e
27	16.6 ee	8.8 ee	7.1	5.7	15.9 eeeeee	5.1
28	8.1	7.5	6.8 e	5.0†	18.8 eeee	6.2
29					9.3 ee	
30					7.8	
Errors	37	10	5	3	27	12

e = Error
* = Lowest record for individual rats
† = Lower record than absolute time value for certain rat
The dotted line divides the table into first and second halves in order to make comparison of the two stages easier. The number of errors made by each rat in each half is shown.

individually the results show that in every case a lower record than the absolute time record was made, but in no case maintained. If the group average be noted, the absolute time is never quite reached, the curve (Fig. 8), starting a little above

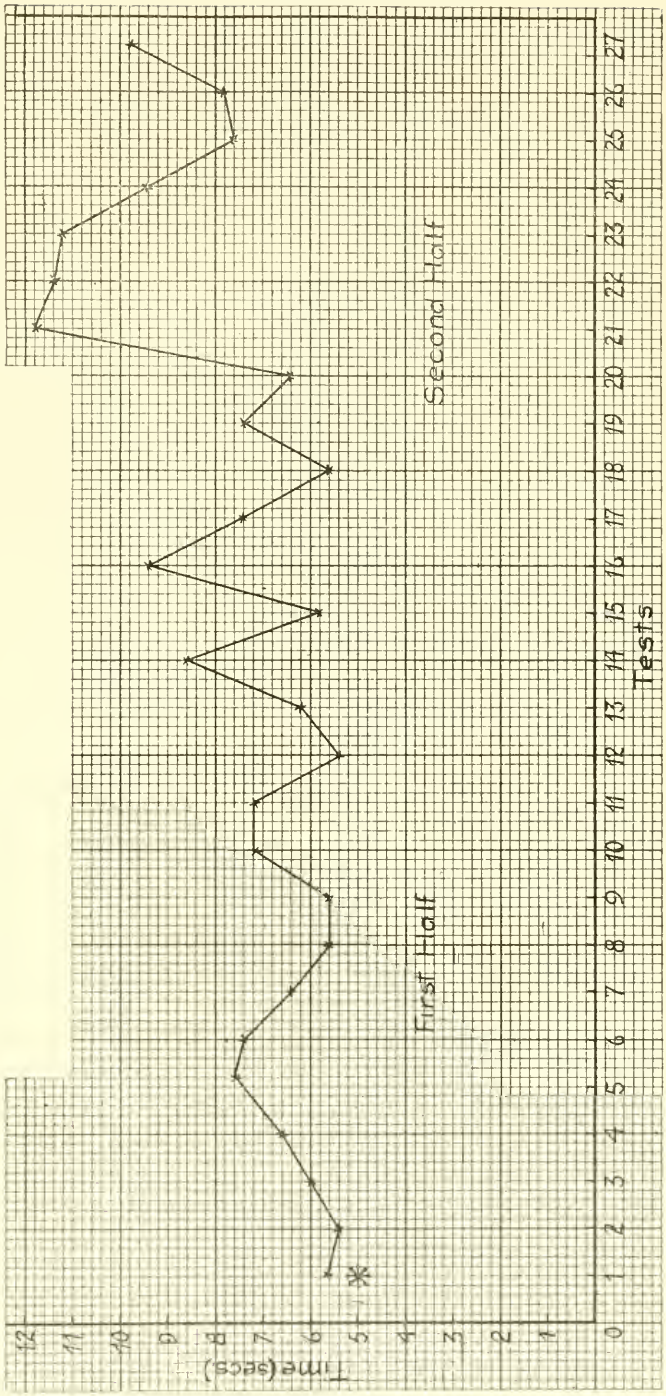

FIGURE 8. Effect of Continuing Trials, on Time Efficiency of Six Sixty-five Day Rats

Each Test Represents the Average for Six Trials. In the Curve 162 Trials are Shown as 27 Tests. The Asterisk (*) Indicates the Point of Efficiency Reached in the Learning Process i.e. the Last Six Trials

it and continually rising. In other words, final efficiency decreases rather than increases when practice is continued. An interesting point is that errors will be made even after the problem is learned. Of the six rats used, three made errors in the first test of six trials after the problem was learned, one in the second test, and one not until the fifth test. Errors increased as the work was continued. During the last half of the one hundred sixty additional trials twice as many errors were made as in the first half.

A closer examination of the table shows: First, that the best record in each case was made during the first fourteen tests; Second, that the last test was better than the first in only one case (rat 15); Third, that rats fourteen and fifteen probably had not reached their efficiency level during the learning process, while the other rats had. This fact is deduced from the number of times each rat made a record lower than its absolute time record.

Rat 8	9 times out of 28
Rat 12	8 times out of 28
Rat 14	21 times out of 28
Rat 15	14 times out of 28
Rat 16	2 times out of 28
Rat 17	3 times out of 28

Blood Relationship and Learning

It was found that the learning ability of certain members of a group could be predicted from the results obtained on other members of the same litter. The data appear in Table XII. Three rats from the Y(CF) litter were used when twenty-five days old, and five rats from the same litter worked when sixty-five days old. Two members of the GJ litter learned the problem at twenty-five days, and one at sixty-five days. Two AL rats worked when twenty-five days old, and one when sixty-five days old. Four XL rats learned the problem at twenty-five days, three at sixty-five days and three at two hundred days.

The rats belonging to the Y(CF) litter required a smaller number of trials at twenty-five days than the average, but their absolute time, total time and total distance were above the average for rats of that age. At sixty-five days, rats of the same litter made averages higher than the group averages for that

age except for the absolute time. The GJ rats twenty-five days old had trial and distance records higher than those of the entire group while their absolute and total times were less. The same holds true for the GJ rats at sixty-five days except that their absolute time is higher. AL rats show records lower than the group averages in every case at twenty-five days but at sixty-five days all of the AL averages are higher than that for the group.

Rats from the XL litter which worked at twenty-five days made lower records than the average except in absolute time. The same is true of the sixty-five day members of the same litter and the two hundred day XL rats have a lower record than the group average in every particular.

In three out of four cases considered then, a high or low average at one age seems to point to the fact that there will be a high or low average for the age or ages following.

It appears that it is possible within limits to predict the capacity for habit formation of rats of a certain litter at a given age, from the behavior of their blood relations at any other age.

TABLE XII

Litter	Age	Trials	Time: Absolute	Time: Total	Distance
YCF	25 days	25	5.9 sec.	463 min.	290.1 m.
	65 "	40	5.9 "	317 "	369.4 "
	200 "	..	..	..	..
GJ	25 "	35	5.3 "	140 "	319.3 "
	65 "	38	7.6 "	136 "	284.0 "
	200 "	..	..	..	..
AL	25 "	23	5.4 "	91 "	185.8 "
	65 "	38	10.6 "	330 "	393.2 "
	200 "	..	..	..	..
XL	25 "	34	5.4 "	180 "	252.5 "
	65 "	24	8.3 "	153 "	195.3 "
	200 "	29	7.8 "	258 "	254.4 "
Gen. Av.	25 "	30	5.7 "	224 "	271.6 "
	65 "	31	6.8 "	318 "	260.6 "
	200 "	42	8.6 "	351 "	339.1 "

RETENTION

A retention test was made on five individuals of the sixty-five day group who were caused to relearn the problem after ninety days. During this time they were fed daily in the maze except

that at the eighty-fifth day the food supply was cut down, and on the eighty-ninth day no food at all was allowed. Probably a better plan would have been to feed the rats in the food box of the maze for a week preceding the retention test, using the same schedule employed in preliminary feeding, and keeping the food box carefully partitioned off from the rest of the maze.

Seventy-six per cent of the original number of trials were required to relearn, forty-eight per cent of the time necessary for learning was occupied in relearning, and fifty-two per cent of the original amount of distance was covered. The absolute time when learning was seven and nine tenths seconds, when relearning nine seconds. This difference probably being due to the increased age, since the rats were approximately two hundred days old at the time of the retention test and absolute time increases with age.

Nothing more is shown by the test on retention than that the interval between learning and relearning must be made very much smaller if it is desired to begin a problem with a view to determining the curve of retention.

The relation of time to distance in learning, and the matter of elimination of alleys in the maze have been discussed at length in papers already published.

TABLE XIII

LEARNING

Rat	Trials	Ab. time	Total time	Distance
1	30	8.2 sec.	1327.4 sec.	23033.6 cm.
2	54	7.8 "	1651.8 "	42368.0 "
3	21	7.1 "	1542.0 "	25177.6 "
4	16	9.8 "	496.0 "	11603.2 "
5	22	6.5 "	2378.0 "	26675.2 "
Totals.....	143	39.5 "	7395.2 "	128857.6 "
Averages....	29	7.9 "	1479.1 "	25771.5 "

RELEARNING—AFTER 90 DAYS

Rat	Trials	Ab. time	Total time	Distance
1	14	7.7 sec.	316.4 sec.	7264.0 cm.
2	40	8.7 "	861.8 "	23321.6 "
3	14	9.2 "	484.4 "	9081.6 "
4	22	10.3 "	958.8 "	15562.6 "
5	18	9.3 "	902.6 "	11426.8 "
Totals	108	45.2 "	3524.0 "	66656.6 "
Averages....	22	9.0 "	704.8 "	13331.3 "

RESUMÉ OF CONCLUSIONS

1. Young rats learn the maze more rapidly than the old ones, the rapidity with which the habit may be formed decreasing with increase in age.

2. Absolute time, the time required for the execution of the perfect run, increases with increase in age, the oldest group requiring more than twice as much time as the youngest.

3. The most rapid stage of habit formation occurs earlier in the learning process of the younger animals than of the older ones.

4. In the very young rats (25 days) and the very old (300 days) sex differences are negligible, while among the animals of medium age (65-300 days) the males learn more rapidly than the females.

5. In general the absolute time for the females is lower than that for the males, suggesting greater efficiency on the part of the former in the execution of the habit when it had once been perfected.

6. Practically no difference in ability to form the maze habit is to be found between rats learning the problem in the daytime and those learning at night.

7. Continued practice after the problem has been learned causes a break in the habit and does not result in an increase of final efficiency.

8. The rapidity with which the maze habit will be formed is predictable, within certain limits, from one family group to another.

9. In the matter of elimination of errors, the outer alleys are usually those in which useless movements are last to drop out, but a 5-4-3-2 order does not hold, i. e., errors in 5 dropping out first, those in 4 second, etc. This bears directly on the question of the relation of the food to the learning process and seems to negate the pleasure-pain hypothesis, but no conclusive evidence has been obtained.

10. The importance of an adequate test on retention is made quite evident by these results.

If an analogy may be drawn between the learning ability of the rat and that of the human subject, it may be seen that in general the old can learn a given problem as well as the young although more effort is required to do so. The efficiency of

this learning can only be measured by testing the retention ability. Should such tests show that the old animals forget very rapidly and must relearn the problem continually with little or no lessening of excess effort, comparing unfavorably with the younger ones in these respects, the above conclusions would have to be modified. If, however, the limits of retention in the groups are found to be very nearly the same, and the amount of effort necessary to relearn not greatly increased for the older group over that for the younger, the deductions would hold.

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